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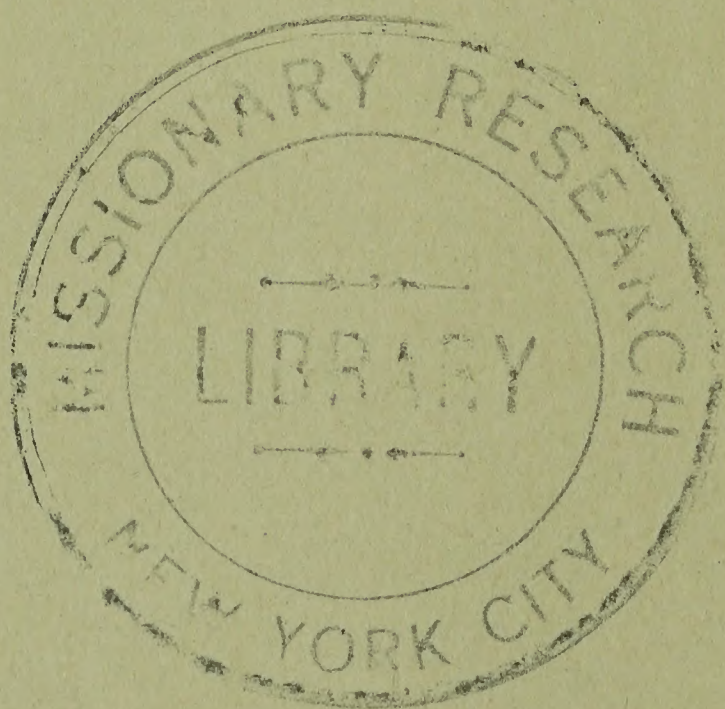
MEMOIR 8
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AMERICAN LINGUISTICS

RESULTS OF THE CONFERENCE OF
ANTHROPOLOGISTS AND LINGUISTS

C. Lévi-Strauss

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Results of the Conference of Anthropologists and Linguists

MEMOIR 8

By

Claude Levi-Strauss

Roman Jakobson

C. F. Voegelin and Thomas A. Sebeok

WAVERLY PRESS, INC.

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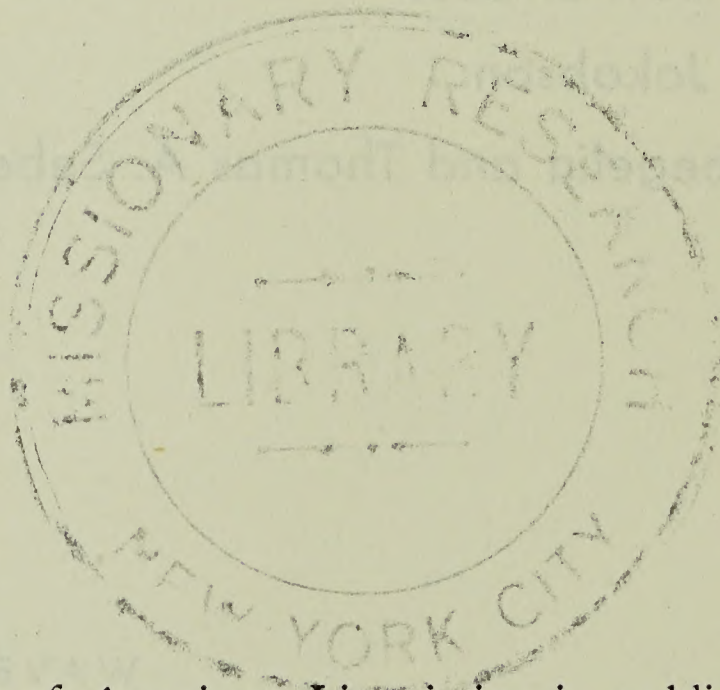
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CONTENTS

| | |
|--|----|
| Preface | v |
| Chapter One | 1 |
| <i>Claude Lévi-Strauss</i> | |
| Chapter Two | 11 |
| <i>Roman Jakobson</i> | |
| Chapter Three | 22 |
| <i>C. F. Voegelin and Thomas A. Sebeok</i> | |



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PREFACE

The sixteen sessions of the Conference of Anthropologists and Linguists were devoted to seven broad topics, followed by a day for integration, as is shown in the schedule below. The Conference was jointly sponsored by the Wenner-Gren Foundation for Anthropological Research and by Indiana University. This seems to be the proper place to record that the administrators responsible, Paul Fejos and John W. Ashton, shared sponsorship of the idea of having an extensive series of conferences between linguists assembled for the summer and cultural anthropologists visiting for the occasion.

Language and Culture. July 21. Afternoon Introducer Henry Lee Smith, Jr., Chairman C. F. Hockett; evening Introducer Alf Sommerfelt, Chairman Fred W. Householder.

Speech and Personality. July 22. Afternoon Introducer Y. R. Chao, Chairman Thomas A. Sebeok; evening Introducer Demitri B. Shimkin, Chairman David Olmsted.

Dialect Distance and Culture History. July 23. Afternoon Introducer W. Freeman Twaddell, Chairman Norman McQuown; evening Introducer C. F. Voegelin, Chairman Louis Hjelmslev.

Bilingualism and Acculturation. July 24. Afternoon Introducer Mary R. Haas, Chairman Archibald A. Hill; evening Introducer Joseph B. Casagrande, Chairman Floyd Lounsbury.

Linguistics and Psychology. July 25. Afternoon Introducer Floyd G. Lounsbury, Chairman Joseph B. Casagrande; evening Introducer Charles E. Osgood, Chairman Thomas A. Sebeok.

Ethnography Without Linguistics and Linguistics Without Meaning. July 28. Afternoon Introducer Verne Ray, Chairman David French; evening Introducer Norman A. McQuown, Chairman David Olmsted.

Language and Thought. July 29. Afternoon Introducer Y. Bar-Hillel, Chairman W. Freeman Twaddell; evening Introducer Rulon Wells, Chairman Verne Ray.

Results of the Conference from the Point of View of Anthropology; Results of the Conference from the Point of View of Linguistics. July 30. Afternoon Introducer Claude Lévi-Strauss, Chairman David Bidney; evening Introducer Roman Jakobson, Chairman John Lotz.

The following papers were prepared for the Conference and distributed among the conferees; these papers are reflected indirectly in our report—i.e., as they influenced the intellectual positions and discussions of the conferees.

Yehoshua Bar-Hillel, *Some Linguistic Problems Connected with Machine Translation*

George C. Barker, *Bilingualism and the Ethnic Group: A Linguistic Approach to Social Differentiation*

R. Birdwhistell, *Introduction to Kinesics*

Zellig S. Harris and C. F. Voegelin, *Eliciting*

Archibald A. Hill, *When is Historical Explanation Relevant?*

C. F. Hockett, *Idiolect, Common Core, and Overall Pattern*

Claude Lévi-Strauss, *Toward A General Theory of Communication*

Charles E. Osgood, *A Psycholinguistic Analysis of Decoding and Encoding Processes*

D. B. Shimkin and Pedro Sanjuan, *Proverbs from Three Russian Rural Districts: A Study in the Sociopsychological Analysis of Linguistic Data*

Henry Lee Smith, Jr., *An Outline of Metalinguistic Analysis*

Rulon Wells, *Predicting Slips of the Tongue*.

Conference participants included: Y. Bar-Hillel, Massachusetts Institute of Technology; George C. Barker, University of California, Los Angeles; Joseph B. Casagrande, Social Science Research Council; Regina Flannery, Catholic University of America; David French, Reed College; Mary R. Haas, University of California, Berkeley; Melville Jacobs, University of Washington; Roman Jakobson, Harvard University; Claude Lévi-Strauss, Sorbonne; John Lotz, Columbia University; Floyd G. Lounsbury, Yale University; Norman A. McQuown, University of Chicago; David L. Olmsted, Yale University; Charles E. Osgood, University of Illinois; Verne Ray, Yale University and Human Relations Area Files; D. B. Shimkin, Harvard University; Alf Sommerfelt, University of Oslo; Rulon S. Wells, Yale University; Gene Weltfish, Columbia University. They also included many faculty members of the Linguistic Institute: Y. R. Chao, University of California, Berkeley; Archibald Hill, University of Virginia; Louis Hjelmslev, University of Copenhagen; C. F. Hockett, Cornell University; Fred W. Householder, Indiana University; Valentin Kiparsky, University of Helsinki; George S. Lane, University of North Carolina; Thomas A. Sebeok, Indiana University; Henry Lee Smith, Jr., Department of State; S. Y. Teng, Indiana University; W. Freeman Twaddell, Brown University; A. P. Ushenko, Indiana University; H. V. Velten, Indiana University; C. F. Voegelin, Indiana University; Harold Whitehall, Indiana University. They further comprised visitors from abroad as well as from various American universities.

Chapters One and Two, which now follow, were read on the last day of the Conference, with titles as given in the preceding schedule. Chapter Three combines some preliminary notes—written in preparation for, and hence before the Conference—with a precis of the Conference itself; this precis is partly interpretive, but largely editorial. The authors-editors of Chapter Three (hereinafter designated as ‘we’) could not have done their work had not John R. Mickey made a tape recording of every word uttered in every one of the sixteen Conference sessions, and had Dell Hymes not provided us with more than adequate notes based on the playback from the tapes. Thus, we had our memory of the Conference sessions refreshed by the sensitive notes written by Hymes which were however something less and, also, something more than a verbatim account of what the conferees said at the actual sessions.

CFV
TAS

CHAPTER ONE

Claude Lévi-Strauss

I think the day has come to congratulate ourselves, and indeed we may do so, since, after all, it has been done. It has been done in spite of many things, and, mostly, in spite of the fact that we are anthropologists and linguists united, probably for the first time on a formal basis and for the specific purpose of confronting together linguistics and anthropology. However, the problem was not a simple one, and it seems to me that some of the many difficulties which we have met with can be referred to the fact we were not only trying to make a confrontation of the theme of linguistics and of anthropology, but that this confrontation itself could be and had to be undertaken on several different levels, and it was extremely difficult to prevent ourselves, in the midst of the same discussion, to shift from one level to another. I shall try first of all to outline what these different levels are.

In the first place, we have spoken about the relation between *a* language and *a* culture. That is, how far is it necessary, when we try to study a culture, to know the language, or how far is it necessary to understand what is meant by the population, to have some knowledge of the culture, besides the language. And several discussions, it seems to me, have specifically referred to this first level.

There was also a second level, which is not any longer the relationship between *a* language and *a* culture, but the relationship between *language* and *culture*. And though there are also, on this level, many important problems, it seems to me that the discussions we had have not so often been placed on the second level, as on the first one. For instance, I am rather struck by the fact that at no moment during our discussions has any reference been made to the behavior of culture as a whole toward language as a whole. Among us, language is used in a rather reckless way—we talk all the time, we ask questions about many things. This is not at all a universal situation. There are cultures—and I would be inclined to say most of the cultures of the world—which are rather thrifty in relation to language. They don't believe that language should be used indiscriminately, but only in certain specific frames of reference and somewhat sparingly. Problems of this kind have, to be sure, been mentioned in our discussion, but certainly were not given the same importance as the problems of the first type.

And there is a third level which received still less attention, though at least one session—the one which was introduced by Lounsbury—was completely devoted to it. It is the relation, not any more between *a* language or *language* and *a* culture or *culture*, but the relation between linguistics as a scientific discipline and anthropology. And this, which to my mind would be probably the most important level, has remained somewhat in the background during our discussion.

Now how can this be explained? The relationship between language and culture is an exceedingly complicated one. In the first place, language can be said to be a result of culture: the language which is spoken by one population is a reflection of the global culture of the population. But also, one can say that language is a *part* of culture. It is one of those many things which make up a culture—and if you remember Tylor's famous definition of culture, culture includes a great many things such as tools, institutions, customs, beliefs, and also, of course, language. And from this point of view the problems are not at all the same as from the first one. In the third place, language can be said to be a *condition* of culture, and this in two different ways: first, it is a condition of culture in a diachronical way because it is mostly through the language that we learn about our own culture—we are taught by our parents, we are scolded, we are congratulated with language. But also, from a much more theoretical point of view, language can be said to be a condition of culture because the material out of which language is built is of the same type as the material out of which the whole culture is built: logical relations, oppositions, correlations, and the like. Language from this point of view may appear as laying a kind of foundation for the more complex structures which correspond to the different aspects of culture.

This is how I see our problem from an objective point of view. And it is not a simple one, as you may see. But there is also a subjective point of view which is none the less important. During the discussion it appeared to me that the reasons why anthropologists on the one hand, and linguists on the other, have been so eager to get together, were of an entirely different nature, and that their motivations were practically contradictory. Linguists have told us over and over again during these sessions that they were somewhat afraid of the trend which is becoming predominant in their discipline—that they felt more and more unrelated; that they were dealing more and more with abstract notions which many times have been very difficult to follow, for the others; and that what they were mainly concerned with, especially in structural linguistics, had no relation whatsoever with the whole culture, with the social life, with the history of the people who speak the language, and so on. And the reason why, it seems to me, linguists were so eager to get closer to the anthropologists was precisely because they expected the anthropologists to be able to give back to them some of this concreteness which has seemed to disappear from their own methodological approach. And now, what about the anthropologists? Oh, the anthropologists are in a very peculiar situation in relation to linguistics. For many years they have been working very close to the linguists, and all of a sudden it seems to them that the linguists are vanishing, that they are going on the other side of the borderline which divides the exact and natural sciences on the one hand from the human and social sciences on the other. All of a sudden the linguists are playing to their former companions this very nasty trick of doing things as well and with the same sort of rigorous approach which for long was believed to be the privilege of the exact and natural sciences. Then, on the side of the anthropologist there is some, let us say, melancholy, and a great deal of envy. We would like to learn

from you how you succeeded to do it, how we may ourselves in our own field, which is a complex one—in the field of kinship, in the field of social organization, in the field of religion, folklore, art, and the like—use the same kind of rigorous approach which has proved to be so successful for linguistics.

And I would like to elaborate—since the point of view that I am expected to explain here is the point of view of the anthropologist—how important this is to us. I learned a great deal during this Conference, but it was not only during the sessions of the Conference: I was extremely impressed, as a non-linguist anthropologist, in attending some of the classes in field work, led by Voegelin and Smith, to witness the precision, the care, the rigor which is used in a field which, after all, belongs to the social sciences to the same extent as the other fields of anthropology. And this is not all. We have also been impressed during the past three or four years not only by the theoretical but by the practical connection which has been established between linguistics and communication engineering—by the fact that now, when you have a problem, it is possible not only to use a method more rigorous than our own to solve it, but to have a machine built by an engineer and to make a kind of experiment, completely similar to a natural science experiment, and that the experiment will tell you if the hypothesis is worth while or not. For centuries, the humanities and the social sciences have resigned themselves to contemplate the world of the natural and exact sciences as a kind of paradise where they will never enter. And all of a sudden there is a small door which is being opened between the two fields, and it is linguistics which has done it. So you may see that the motivations of the anthropologists, insofar as I am able to interpret them correctly, are rather contradictory to the motivations of the linguists. The linguists try to join the anthropologists in order to make their study more concrete, while the anthropologists are trying to rejoin the linguists because, precisely, the linguists appear to show them a way to get out of the confusion resulting from too much acquaintance and familiarity with the concrete and empirical data. Sometimes it seems to me this has resulted, during this Conference, in a somewhat—shall I call it—unhappy merry-go-round, where anthropologists were running after the linguists while the linguists were running after the anthropologists, each trying to get of the other precisely what he was trying himself to get rid of. And this, I think, deserves some kind of attention. Why this basic misunderstanding? In the first place, because the task is extremely difficult. I was particularly struck by the session where Haas tried to write on the board formulas to analyze a problem as apparently simple as that of bilingualism—very simple, since it might seem from the outside that there are only two terms, two languages, though the number of possible permutations were enormous. And enormous as they were, during the discussion new types of permutation were discovered. It was also admitted that, besides these permutations, dimensions could be introduced, which would still complicate the problem much more. This is what I believe one of the main teachings of this Conference—that whenever we try to express in the same language linguistic problems and cultural problems, the situation becomes tremendously complicated and we shall always have to keep this in mind.

In the second place, we have been behaving as if there were only two partners—language on the one hand, culture on the other—and as if the problem should be set up in terms of the causal relations: “Is it language which influences culture? Is it culture which influences language?” But we have not been sufficiently aware of the fact that *both* language and culture are the products of activities which are basically similar. I am now referring to this uninvited guest which has been seated during this Conference beside us and which is *the human mind*. Uninvited, certainly, but the fact that Osgood was there, and the many occasions when he was literally compelled to intervene in order to call our attention to this basic fact, is the best proof of the point I am trying to make.

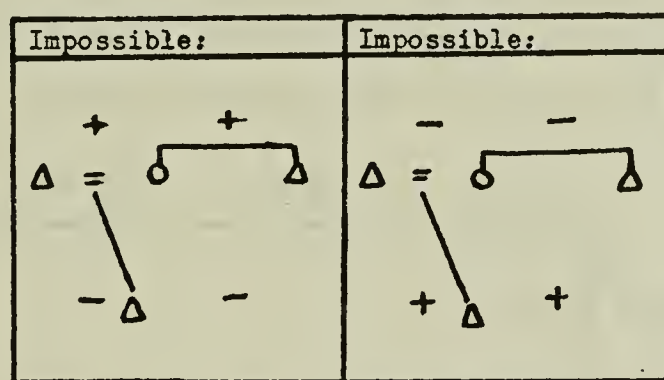
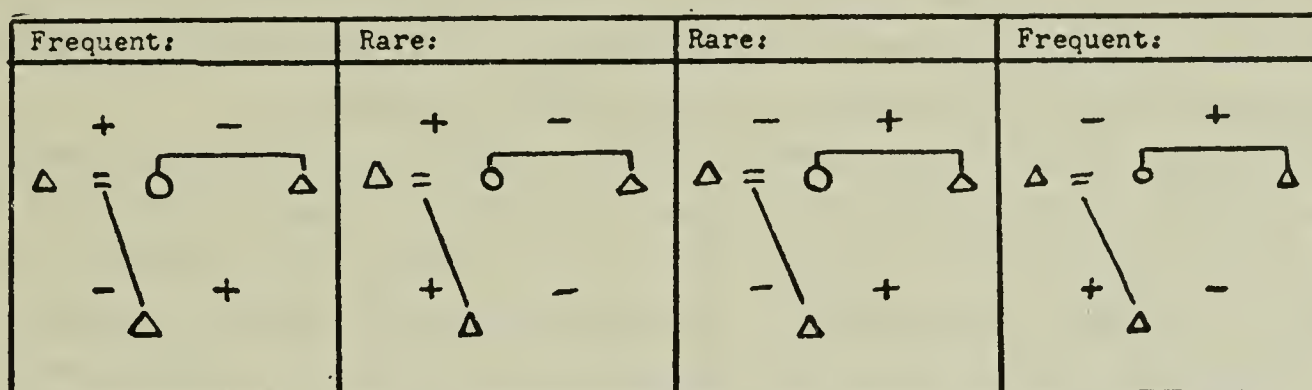
If we try to formulate our problem in purely theoretical terms, then it seems to me that it is obvious that we are entitled to affirm that there should be some kind of relationship between language and culture, because language has taken thousands of years to develop and culture has taken thousands of years to develop and both processes have been developing side by side within the same minds. Of course, I am leaving aside for the moment the cases where a foreign language has been adopted by one culture, or else a foreign culture by people having previously a language—we can, for the sake of argument, consider only those cases where, in an undisturbed fashion, language and culture have been able to develop together. Is it possible to conceive of the human mind as consisting of compartments separated by rigid bulkheads without anything being able to pass from one bulkhead to the other? Though, when we try to find out what these connections or correlations are, we are confronted with a very serious problem, or, rather, with two very serious problems.

The first problem has to do with *the level* which we try to correlate, and the second one, with *the things* we try to correlate. I shall now give some attention to these basic distinctions.

I remember a very striking example which was given to us by Lounsbury, about the use of two different prefixes for the womankind among the Oneida. Lounsbury was telling us he paid great attention to what was going on on the social level, but he could find no correlation whatsoever. Indeed, no correlation can be found on the level of behavior because behavior, on the one hand, and categories of thought, on the other, such as would be called for to explain the use of these two different prefixes, belong to two entirely different levels. It would not be possible to try to correlate one with the other.

But I can hardly believe it a pure coincidence that this strange dichotomy of the womankind should appear precisely in a culture where the maternal principle has been developed in such an extreme way as among the Iroquois. It is as if the culture had to pay a price for giving the women an importance elsewhere unknown, the price being an inability to think of the women as belonging to only one logical category. To recognize them, unlike most cultures of the world, as full social beings would thus compel the culture, in exchange, to assimilate this part of the womankind which is not as yet able to play that important role—such as young girls—to animals and not to humans. However, when I suggest

this interpretation, I am not trying to correlate language and behavior, but two parallel ways of categorizing the same data.

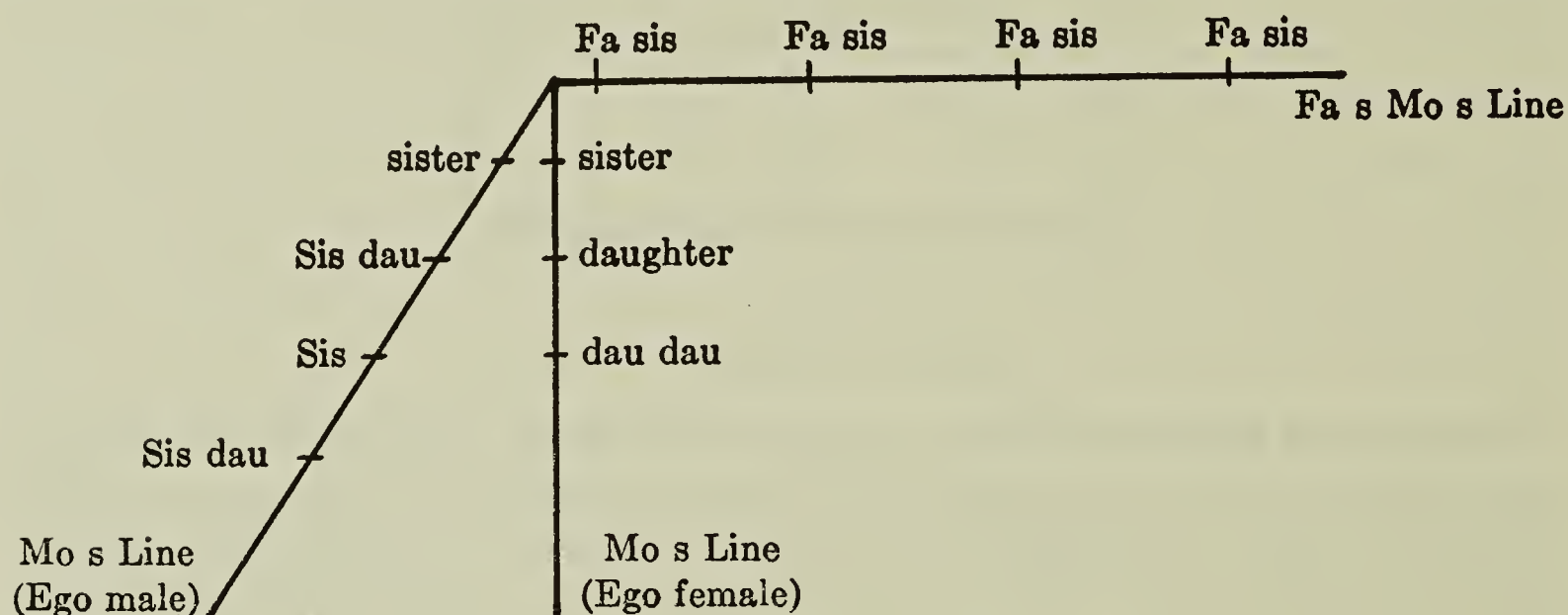


Structures of Behavior

Let me now give you another example. We reduce the kinship structure to the simplest conceivable element, the atom of kinship, if I may say so, when we have a group consisting of a husband, a woman, a representative of the group which has given the woman to the man—since incest prohibitions make it impossible in all societies that the unit of kinship can consist of one family, it must always be across two families, two consanguineous groups, and one offspring. Now it can be shown that, if we divide all the possible behavior between kin, according to a very simple dichotomy, positive behavior and negative behavior (I know this is very unsatisfactory, but it will help me to make my point), it can be shown that a great many different combinations can be found and illustrated by specific ethnographical observations. We will find a positive attitude between husband and wife, and a negative attitude between brother and sister. This can be correlated with (+) between father and son and (−) between maternal uncle and nephew; or it can be the other way around without anything being changed at this parental level; or it can be like this: ($\overline{+}$ or $\overline{+}$), with an inversion of terms on this level. There are four possible permutations. What is absolutely impossible is either this: ($\overline{++}$ or $\overline{++}$); or the opposite, that is, two plus signs on one generation level and two minuses on the other generation level. This is impossible because, in one case, the group will break down synchronically, and, in the other case, it will break down diachronically. Now, what connections are possible with linguistics? I cannot see any whatsoever, except only one, that when the anthropologist is working in this way he is working

more or less in a way parallel to that of the linguist. They are both trying to build a structure with constituent units. But, nevertheless, no conclusions can be drawn from the repetition of the signs in the field of behavior and the repetition, let's say, of the phonemes of the language, or the grammatical structure of the language; nothing of the kind—it is perfectly hopeless.

Now let's take a somewhat more elaborate way of approaching a problem of that kind, Whorf's approach, which was discussed so many times and which certainly was behind the minds of most of us during this discussion. Whorf has tried to establish a correlation between certain linguistic structures and certain cultural structures. Why is it that the approach is unsatisfactory? It is, it seems to me, because the linguistic level as he considered it is the result of a rather sophisticated analysis—he is not at all trying to correlate an empirical impression of the language, but rather the result of true linguistic work (I don't know if this linguistic work is satisfactory from the point of view of the linguists,



I'm just assuming it for the sake of argument)—what he is trying to correlate with this linguistic structure is a crude, superficial, empirical view of the culture itself. So he is really trying to correlate things which belong to entirely different levels.

When we now turn to study the communication system, there are two statements that can be made. The first one is that in order to build a model of the Hopi kinship system one has to use a block model, tri-dimensional. It is not possible to use a two-dimensional model. And this, incidentally, is characteristic of all the Crow-Omaha systems. Now, why is that so? Because the Hopi system makes use of three different time-continuums. We have the first one which corresponds to the mother's line (Ego female), which is a kind of time-dimension which we use ourselves, that is, progressive and continuative: we have the grandmother, mother, Ego, daughter, granddaughter, and so on; it is really genealogical. Now, when we consider other lines, there is a different time dimension: for instance, if we take the father's mother's line, we find that, although people do belong to generations which are consecutive to each other, the same terms are consistently applied to them—that is, a woman is called "father's sister" and her daughter is still "father's sister", and so on indefinitely; this is a

kind of empty time, with no change taking place whatsoever. And there is also a third dimension, which is found in the mother's line for Ego male, where we find a kind of undulation—individuals are alternately called "sibling" or "nephew".

Now if we consider the Zuni kinship system, these three dimensions still exist, but they are considerably reduced, they have a somewhat abortive form. And what is important is that the "straight" time framework which we have in the mother's line is replaced by a kind of "circular" framework, where we have only three terms, a term which means equally well "grandmother" and "grandchild", and then a term for "mother" and a term for "daughter"—a woman would call by the same term her grandmother and her granddaughter.

If we look now at another Pueblo system, let us say Acoma or Laguna, which are Keresan and belong to a different linguistic stock, then we find a completely new picture: the development of symmetrical terms. Two individuals who occupy symmetrical positions in relation to a third individual will call themselves by the same term. This is usually called a "self-reciprocal" terminology.

When we pass from Hopi to Acoma, we have a change from a block model to a flat model, but we have other significant changes. We have a change from a time framework which has a threefold quality, through the Zuni which is intermediate, to something which is quite different: it is not any more a time-dimension, it is a time-space dimension, since in order to conceive of the system one individual has to think of the other individual through the intermediary of a third one.

This can be very well correlated with the different aspects of the same myths among the Hopi, Zuni, and Acoma. When we consider one myth, let us say the emergence myth, the very striking thing is that among the Hopi the entire structure of the myth is organized in a genealogical way. The different gods are conceived as husband, wife, father, grandfather, daughter, and so on, to each other, more or less as it occurs in the Greek pantheon. Among the Zuni we do not find such a developed genealogical structure. Instead we find a kind of cyclical historical structure. The history is divided into periods, and each period repeats to some extent the preceding period. Now, at Acoma the striking fact is that most of the characters which among the Hopi or the Zuni are conceived as one person are dichotomized into different persons with antithetic attributes. This is made clear by the fact that the emergence scene which is so obvious in the first two cases, is preceded, and to some extent replaced, by a dual operation where the power from above and the power from below cooperate to create mankind. It is not any more a progressive linear movement, it is a system of polar oppositions such as we find in the kinship system. Now if it is true that these features of the kinship system can be correlated with systems belonging to a completely different field, the field of mythology, we are entitled to ask the linguist whether or not something of the same kind does not show in the field of language. And it would be very surprising if something—I don't know exactly what, because I am not a linguist—could not be shown to exist, because if the answer should be in the negative, we should have to assume that while fields

that are so widely apart as kinship and mythology nevertheless succeed in remaining correlated, language and mythology, which are much more closely correlated, show no connection or no communication whatsoever.

This new formulation of the problem is, it seems to me, on a level with what the linguists are doing. The linguists are dealing in grammar with the time aspect. They discover the different ways of expressing the idea of time in a language. And it should be allowed to try to compare the way of expressing time on a linguistic level with the way of expressing time on the kinship level. I don't know what the answer will be, but it is possible to discuss the problem, and it is possible in a meaningful way to answer it by "yes" or "no."

Permit me to give you another, and much more elaborate, example of the kind of analysis which the anthropologist could perform to try to find common ground with the linguist. I am going to consider two social developments which have taken place in widely different parts of the world, the first one in an area extending roughly from India to Ireland, the second one in an area extending from Manchuria to Assam. I am not at all saying that each one of these areas has shown exactly one kind of development, and only one. I'm just saying that the developments I'm referring to are well illustrated within these very vague boundaries which, as you are well aware, correspond to some extent to the boundaries of the Indo-European languages on the one hand and the Sino-Tibetan languages on the other.

I propose to consider from three different points of view what has taken place. First, the marriage rules; next, social organization; and third, the kinship system.

| | INDO-EUROPEAN AREA: | SINO-TIBETAN AREA: |
|---------------------|--|--|
| Kinship System | 1. Subjective 2. Few terms | 1. Objective 2. Numerous terms |
| Marriage Rules | Circulative systems either expressed directly in rules or resulting indirectly from the fact that the choice of a mate is left to probability. | Circulative systems present in juxtaposition with exchange systems. |
| Social Organization | Numerous social units with a complex structure (type: extended family) | Few to numerous social units with a simple structure (type: clan to lineage) |

Now let's consider first the marriage rules, for the sake of clarity. What we find in the Indo-European area are various systems which, in order to be properly interpreted, have to be referred to a very simple type of marriage rule which has been called the general form of exchange, or circulative system, because any number of groups can be connected by using this rule. This corresponds roughly to what the anthropologists have called marriage with mother's brother's daughter: group A is taking wives from group B, group B from group C, and group C again from group A, so it's a kind of circle; you can have two groups, three groups, four, five, any number of groups; they can always be organized according to this system. This does not mean that Indo-European speaking

groups have necessarily, at one time or another, practiced marriage with mother's brother's daughter but that most marriage systems in their area of occupancy belong directly or indirectly to the same family as the simpler type.

Now, in the field of social organization, what do we have? We have, as distinctive of the Indo-European area, something which we know by the name of "extended family." What is an extended family? An extended family consists of several collateral lines; but these collateral lines should remain to some extent distinguished from each other, because if they should not—if, for instance, extended family A were marrying into B, extended family B into C, then there would be no distinction whatsoever between an extended family and a clan. The extended family would become a kind of clan. And what keeps the different constituting lines distinct in an extended family is that there cannot exist a rule of marriage applicable to all the lines. Now this has been followed up in Indo-European kinship systems in many different ways. Some systems, which are still working in India, state that it is only the elder line which follows a rule, and that all the other lines can marry exactly as they wish within the only limitation of prohibited degrees. When one studies certain curious features of the old Slavic kinship system, the interpretation is somewhat different: it seems that what may be called the "exemplary line" was more or less diagonal to the main one, that is, if a man would marry according to a given rule, then at the next generational level it will be a man of a different line, and then at the next generation a man of another different line. This does not matter. The point is that with an extended family system it is not possible for all the groups to marry according to the same rule, and that a great many exceptions to any conceivable rule should take place.

Now the kinship system itself calls for very few terms and it is a subjective system. This means that all the relations are described in relation to the subject, and the farther the relation is from the subject the vaguer the terms are. We can accurately describe our relationship to our father, mother, son, daughter, brother, and sister, but aunt or uncle are already more vague; and when it comes to more distant relationships, we have no terms at all at our disposal; it is an egocentric system.

Let's now compare some features in the other area. Here we find two types of marriage rules: one which is the same as the one previously described, general exchange; and another one, which is a special form of exchange, usually called "exchange marriage," a more special form because, instead of making it possible to organize any number of groups, it can only work with two, four, six, eight, an even number—the system couldn't work with an odd number. And these two rules exist side by side within the second area.

Now about the social organization. We don't have extended families, but we find very simple types of the clan system which can become complicated quantitatively (when the clan system divides into lineages), but never qualitatively as is the case with the extended family.

As regards the kinship system, the terms are very numerous. You know, for instance, that in the Chinese kinship system the terms number several hundreds.

and it is even possible to create an indefinite number of terms; any relationship can be described with accuracy even if it is very far away from the subject. And this makes the system completely objective; as a matter of fact, Kroeber has noticed a long time ago that no kinship systems are as completely different from each other as the Indo-European on the one hand and the Chinese on the other.

If we try to interpret this picture, what do we find? We find that in the Indo-European case we have a very simple structure (marriage rules), but that the elements (social organization) which must be organized in this structure are numerous and complicated, whereas in the Sino-Tibetan case the opposite prevails: we have a very complicated structure (marriage rules) with two different sets of rules, and the elements (social organization) are few. And to the separation between the structure and the elements correspond, on the level of terminology—which is already a linguistic level—antithetic features as to the framework (subjective versus objective) and to the terms themselves (numerous versus few). Now it seems to me that if we formulate the situation in these terms, it is at least possible to start a useful discussion with the linguists. While I was making this drawing, I could not but remember what Jakobson was saying at yesterday's session about the structure of the Indo-European language: a great discrepancy between form and substance, a great many irregularities in relation to the rules, and many other contrasts that probably he would be able to point out better than I can.

Finally, I would say that between culture and language there cannot be *no* relations at all, and there cannot be hundred percent correlation either. Both situations are impossible to conceive. If there were no relations at all, that would lead us to assume that the human mind is a kind of mess—that there is no connection at all between what the mind is doing on one level and what the mind is doing on another level. But, on the other hand, if the correlation were hundred percent, then certainly we would know about it and we would not be here to discuss whether it exists or not. So the conclusion which seems to me the more likely is that some kind of correlation exists between certain things on certain levels, and our main task is to determine what are these things and what are these levels. This can only be done through a close cooperation between linguists and anthropologists. But I would say that the most important results of such cooperation will not be for linguistics alone or for anthropology alone, or for both; they will mostly be for an anthropology conceived in a wider way—that is, a knowledge of man which incorporates all the different approaches which can be used and which will provide a clue to the way according to which our uninvited guest, the human mind, works.

CHAPTER TWO

Roman Jakobson

I might say that I liked everything at this Conference. There is only one negative side for me: I have to sum up its linguistic results. First, I could begin with the statement that the Conference was extremely successful. However, I have studied the theory of communication and I know that a statement contains information only when there is a two choice situation. But for a man who closes a Conference there is no two choice situation: it can never be heard from him that the Conference was not successful.

I should like to present all the linguistic results of this Conference as I see them. Of course, I will interpret them and shall not be the translation machine that, as our friend Bar-Hillel so excellently showed, does not understand and therefore translates literally. Once there is interpretation, there emerges the principle of complementarity promoting an interaction between the tool of observation and the thing observed. I will try, however, to be as objective as I can.

What in my opinion is the most important result of this Conference? What has struck me? First, the great unanimity. There was an amazing unanimity. Of course, when I say unanimity, it doesn't mean uniformity. You see, it was a polyphonic structure. All of us here—I might say—sounded differently, but we all were like allophones assigned to one and the same phoneme.

Of course, the most symptomatic fact was a clear-cut liquidation of any kind of isolationism, and isolationism is just as hateful in scientific as it is in political life. There were no longer any such slogans as Linguistics versus Anthropology, Linguistics of the Western Hemisphere versus Linguistics of the Eastern Hemisphere, Formal Analysis versus Semantics, Descriptive Linguistics versus Historical Linguistics, Mechanistic View versus Mentalism and so on. This does not mean that there are no tasks of specialization and that there is no need of focusing upon certain limited problems, but these are mere ways of experimentation and not different biases. As was expressed here really beautifully, we can't actually isolate elements but only discriminate them. If we treat them separately in the process of linguistic analysis, we must continue to remember the artificial character of such a separation. We can discuss language on the morphemic level without reference to the phonemic level. We can discuss the formal level without reference to the semantic level and so on. But we understand that when we do so, it is simply like an acoustical filtering—we can exclude high frequencies or, on the contrary, low frequencies, but we know that it is only a method of scientific experiment. It is likewise very interesting to observe blind-man's buff: how does a person act when blindfolded? What can we say about language when we don't know its meanings? Again, it is very instructive to observe a person run when his movements are hampered as in the so-called sack races, but no one will say that it is more efficient to run with a sack than with-

out one. Thus we realize ever more clearly that our optimum goal is the observation of language in all its complexity. To paraphrase Terence's saying, *Linguista sum: linguistici nihil a me alienum puto*.

Now, if we study language together with anthropologists, their help is most welcome and stimulating, because again and again anthropologists repeat and prove that language and culture imply each other, that language shall be conceived as an integral part of the life of society and that linguistics is closely linked to cultural anthropology. I need not discuss here this relation which was so revealingly presented by Lévi-Strauss. But I should like to second Bidney in what he contributed to the afternoon discussion: there is an even closer genus to embrace the species *language*. Language is an instance of that subclass of *signs* which under the name of *symbols* have been astutely described by Chao who indeed symbolically embodies the best in both Western and Eastern thought. Therefore, when specifying language we must, with Smith, observe other symbolic patterns for a comparison, the system of gestures, for instance, so stimulatingly tackled by Kuleshov, Critchley, and now by Birdwhistell. It presents, I agree, instructive similarities to language and—let us add—not less prominent differences. In the impending task of analyzing and comparing the various semiotic systems, we must remember not only the slogan of de Saussure that linguistics is a part of the science of signs but, first and foremost, the life-work of his no whit less eminent contemporary and one of the greatest pioneers of structural linguistic analysis, Charles Sanders Peirce. Peirce not only stated the need of semiotics but drafted, moreover, its basic lines. His fundamental ideas and devices in the theory of symbols, and particularly of linguistic symbols, when carefully studied, will be of substantial support for the investigation of language in its relation to the other systems of signs. We will then be able to discern the peculiar features of a linguistic sign. Now, one can but agree with our friend McQuown who realized perfectly that there is no equality between systems of signs, and that the basic, the primary, the most important semiotic system is language: language really is the foundation of culture. In relation to language other systems of symbols are concomitant or derivative. Language is the principal means of informative communication.

For the study of language in operation, linguistics has been strongly bulwarked by the impressive achievement of two conjoined disciplines—the mathematical theory of communication and information theory. Although communication engineering was not on the program of our Conference, it is indeed symptomatic that there was almost not a single paper uninfluenced by the works of Shannon and Weaver, of Wiener and Fano, or of the excellent London group. We have involuntarily discussed in terms specifically theirs, of encoders, decoders, redundancy, etc. What, precisely, is the relation between this communication engineering and linguistics? Is there perhaps some conflict between these two approaches? Not at all! As a matter of fact, structural linguistics and the research of communication engineers converge in their destinations. Then what, actually, is the use of communication theory for linguistics and vice versa? We must confess that, in some respects, the exchange of information has found on the part

of the engineers a more exact and unambiguous formulation, a more efficient control of technique used, as well as a promising possibility of quantification. On the other hand, the immense experience of linguists with language and its structure permits them to expose the inconsistencies and failings of the engineers when dealing with linguistic material. Along with the cooperation of linguists and anthropologists, I believe that most productive will be a consistent cooperation of linguists, and perhaps of anthropologists too, with the communication engineers.

Let us analyse the basic factors participating in linguistic communication: any speech event involves a message and four items connected with it—the sender, the receiver, the topic of the message, and the code used. The relation between these four items is variable. Sapir analysed the linguistic phenomena prevalently from the point of view of their ‘cognitive function,’ which he conceived as the primary function of language. But this emphasis of the message on its topic is far from being the only possibility. At present, the emphasis of the message on its other factors begins to attract a higher attention among linguists both in this country and abroad, in particular the emphasis on the communicators—the sender and receiver. Thus we welcome Smith’s keen observations of those linguistic components which serve to characterize the speaker and his attitude to what he is speaking about and toward the listener.

Sometimes these different functions act separately but normally there appears a bundle of functions. Such a bundle is not a simple accumulation but a hierarchy of functions and it is very important to know what is the primary and what the secondary function. I was pleased with all the stimuli which I found on this problem in Smith’s paper. I shall, however, not use his very rich terminology. I must confess that I agree on this point with Ray. New terms are very often a children’s disease of a new science or of a new branch of a science. I now prefer to avoid too many new terms. When we discussed phonemic problems in the twenties, I myself introduced many new terms and then I was by chance liberated from this terminological disease. When I was in Sweden, Collinder, who dislikes phonemics, said that he would like me to do a book for the Linguistic Society of Uppsala: “only, please, no phonemics!” I was just completing my book on the phonemics of children’s language and aphasia, and I simply eliminated the phonemic terms, upon which he said: “now it’s fine!” The book was, in fact, widely understood and I, in turn, understood that it was possible, even in discussing totally new problems, to emancipate the work from new terms. I don’t care whether I say “linguistics” and you say “micro-linguistics.” I call the different sections of linguistics by traditional terms—you prefer the compounds “microlinguistics” and “metalinguistics.” Although the traditional terms are perfectly satisfactory, “microlinguistics” does no harm. The coinage “metalinguistics” is—I agree with Chao and others—a little bit dangerous, because metalinguistics and meta-language mean other things in symbolic logic. Since it is better to have unclouded interdepartmental relations with logicians, one had rather avoid such ambiguities. Besides, you would be surprised if a zoologist, in describing what a certain animal eats and in what part of the world it is to

be found, would call such questions meta-zoology. But I don't object—I still follow my late teacher Peshkovskij who said, "Let's not quibble about terminology; if you have a weakness for new terms, use them. You may even call it 'Ivan Ivanovich,' just so we all know what you mean."

Returning now to the linguistic functions—I mentioned the emphasis on the topic, on the sender, on the receiver; and we see how many new things we are able to do when analysing this paramount problem of sender and receiver. Moreover, there is the possibility of an emphasis either on the code or on the message. This emphasis of the message on its own self is called poetic function. I am very happy that, if not at this Conference at least at the next, as it was said, this function will form part of the discussion. Hill's and Whitehall's successful seminar on poetic language at this Linguistic Institute is one of the eloquent proofs that the problems of poetic language enter into the foreground of American linguistics. I am happy that, as Whitehall intimates in his excellent pamphlet recently published by the Foreign Service Institute, a bridge is finally being built between linguistics and literary criticism in this country. The proper subject of inquiry into poetry is precisely language, seen from the point of view of its preponderant function: the emphasis on the message. This poetic function, however, is not confined to poetry. There is only a difference in hierarchy: this function can either be subordinated to other functions or appear as the organizing function. The conception of poetic language as a language with a predominant poetic function will help us in the understanding of the every-day prosaic language where the hierarchy of functions is different but where this poetic (or aesthetic) function necessarily exists and plays a palpable role both in the synchronic and diachronic aspect of language. There are instructive border cases: the biggest linguistic *code unit* functions at the same time as the smallest *poetic whole*, and in this marginal area the research of my friend Shimkin on proverbs is a fascinating theme, since the proverb is both a phraseological unit and a poetic work.

We mentioned the factors involved in a speech event but we did not touch upon their interchangeability, the roles of sender and receiver merging or alternating, the sender or the receiver becoming the topic of the message, and the other interactions of all these factors. The most essential problem for speech analysis is that of the code common to both sender and receiver and underlying the exchange of messages. No communication is feasible without a certain stock of what the engineers, and especially MacKay, one of the nearest to linguists among them, call preconceived possibilities and prefabricated representations. And when I read all that was written by the communication engineers, especially American and English (in particular Cherry, Gabor, and MacKay), on message and code, I realized of course that both these conjoined aspects have been for a long time familiar to the linguistic and logical theories of language here and abroad under various dichotomous names such as *langue—parole*, *Language—Speech*, *Linguistic Pattern—Utterance*, *Legisigns—Sinsigns*, *Type—Token*, *Sign-design—Sign-event*, etc.; but at the same time I must confess that the *Code—Message* concepts of communication theory are much clearer, much less ambigu-

ous, and much more operational than the traditional presentation of this dichotomy in the theory of language. I believe that it's preferable to work at present with these well-defined, measurable and analysable concepts without replacing them by new, once again somewhat vague terms such as the 'common core'.

Communication theory seems to me a good school for present-day linguists, just as structural linguistics is a useful school for communication engineering. I think that the basic reality facing a linguist is the interlocution—the exchange of messages between sender and receiver, between addresser and addressee, between encoder and decoder. There occur attempts to revert to a very, very old, I should say pre-Whitneyan, stage of our science in considering individual speech as the only reality. As I already mentioned, individual speech doesn't exist without an exchange. There is no sender without a receiver—oh, yes, there is, only if the sender is drunk or pathological. As to non-exteriorized, non-uttered, so-called inner speech, it is only an elliptic and allusive substitute for the more explicit, enunciated speech. Furthermore, dialogue underlies even inner speech, as demonstrated from Peirce to Vygotskij.

With the customary great interest I read the paper on *Idiolect*, distributed by my old friend Hockett. This paper confines the idiolect to a single individual's habits of speaking at a given time, not including his habits of understanding the speech of others. If my Cambridge utterances over a longer period were observed and tape-recorded, one would never hear me use the word 'idiolect.' And nevertheless now, when speaking with you, I use it because I am adapting myself to my potential opponents, for instance, to Hockett. And I use many other terms in this same way. Everyone, when speaking to a new person, tries, deliberately or involuntarily, to hit upon a common vocabulary: either to please or simply to be understood or, finally, to bring him out, he uses the terms of his addressee. There is no such thing as private property in language: everything is socialized. Verbal exchange, like any form of intercourse, requires at least two communicators, and idiolect proves to be a somewhat perverse fiction.

There are indeed many stimuli to be gained for linguists from the theory of communication. A normal communication process operates with an encoder and decoder. The decoder receives a message. He knows the code. The message is new to him and, by virtue of this code, he interprets the message. To comprehend this operation we now have the great help of psychology. One of the most pleasant experiences we had during this conference was Osgood's astute report on the psycholinguistic analysis of decoding and encoding processes.

The receiver understands the message from the code. The position of the linguist who deciphers a language he doesn't know is different. He tries to deduce the code from the message: thus he is not a decoder; he is what is called a cryptanalyst. The decoder is a virtual addressee of the message. The American cryptanalysts who, during the war, read the Japanese secret messages were not the addressees of these messages. Obviously, the linguist must develop the technique of cryptanalysts; and, naturally, when one deals too long a time with a technique, one begins to believe that it is the normal procedure. But, as a matter of fact, such a procedure is quite marginal and exceptional in usual communication, and

even the task of a linguist is to begin with the job of the cryptanalyst but to end up as the normal decoder of this language. His ideal is to become like a member of the studied speech community. The cryptanalyst observes allophones and looks for the phonemes. But the phonemes, the invariants, are much more intimately known to the decoder, the member of the speech community, than are the variations. He doesn't care what the allophones are. He wants to pick out the phonemic contrasts in order to understand the text. (By the way, the terms 'allophone' and 'contrast' are in my parlance further examples of the verbal adaptation of the speaker to his listeners; otherwise, I would say 'variant' and 'opposition'.)

In this field of interaction between message and code, this Conference shows great progress. We have discussed here, on various levels, the relation between two participants of speech communication. And as we too well know, one of the essential duties of language is to bridge space—to span distance—to create a spatial continuity—to find and establish a common language through the air. Of course, if distance is at stake, there emerge still greater and more numerous dialectal differences. If there are two neighboring speech-communities, the code is not the same, but still there is no hermetic isolation of either speech community. This may occur only as an abnormal, rather pathological case. As a rule, there is a tendency to understand the members of the other speech community, and we heard the illuminating paper of my tried friend Twaddell which showed us how such a mechanism works. This is the 'code switching' of the communication engineers. Twaddell always senses not only the problem of present-day linguistics but also the problem of tomorrow. Just as his monograph on defining the phoneme was a spur to a search for a strictly scientific phonemic analysis, his new paper calls for thorough attention to the focal linguistic problem of code-switching.

We proceed now to the puzzles of bilingualism, graphically discussed by Haas and Casagrande. We are still on the same problem of bridging space. Here almost nothing stems from a common core. The codes become still more and more different. But there is always a certain correspondence, a certain relation between the two codes. There is the possibility of a search for at least a partial understanding, and there are in such relations interlingual mediators, interpreters—bilingual people. Here we reach a very relevant, decisive point. Bilingualism is for me the fundamental problem of linguistics, because the division into departments was artificial—the department of French, the department of Italian, etc. Are the contiguous languages in complete segregation? If there is an iron curtain, we know how easily such a curtain is penetrated by various forms of speech communication. We know that there exist bilingual areas or bilingual groups of speakers, and the sociology of language presents us with interesting accounts of them. Since bilingual people can obviously speak to and influence a higher number of listeners, they consequently have a higher power, a higher prestige. What is then the result? There is an adaptation on the part of the bilingual person from one language to another and a subsequent diffusion of certain phenomena stimulated by bilingual people among non-bilingual people. As it

was pointed out in that most important paper of Sommerfelt's, we face the question of the diffusion of patterns—of phonemic patterns, of grammatical categories, of what Sapir called the grammatical processes. And we will see how enormous this diffusion is when we obtain the Atlas begun in Oslo before the last war, the atlas of such phenomena, cartographed regardless of the boundaries and relationships of the languages carrying these phenomena. I spoke with one of the most sober among linguists, Haas, and with one of the most sober among anthropologists, Ray. The extent of such a phonemic and grammatical diffusion among neighboring languages of clearly different origin appeared to us so surprising, so difficult to explain that we were unanimous in stressing the urgent need of a systematic, international study of these phenomena. This task does not at all eliminate the problem of genetic kinship, but the problem of affinity is no less important—and, without knowing exactly what affinity is, we will never detect the genetic features.

So much for space. Now we must confront the factor of time. It was not discussed at this Conference but it was examined in the brilliant mimeographed paper by Hill, distributed here. We were accustomed to textbooks advocating a complete split between synchronic and diachronic linguistics. They were presented as two entirely diverse methodologies, two basically different problems. This is, in my opinion, obsolete and we are in complete agreement with the views of Hill: the history of a language can only be the history of a linguistic pattern, linguistic system, which undergoes different mutations. Each mutation must be analysed from the point of view of the pattern as it was before the mutation and after it. And here we come to an important point. I formulate it in other terms than Hill, but I hope that we will be no less in accord. It seems to me that the great mistake and confusion, the sharp separation between synchrony and diachrony was to a high degree due to a confusion between two dichotomies. One is the dichotomy of synchrony and diachrony, and the other is the dichotomy between static and dynamic. Synchronic is not equal to static. When at a movie I ask you what you see at a given moment on the screen, you won't see statics—you'll see horses running, people walking, and other movements. Where do you see static? Only on the billboards. On the billboard it is static but not necessarily synchronic. Suppose a billboard remains unchanged for a year that is static. And it is completely legitimate to ask what is static in the linguistic diachrony. I'm sure it would interest Hahn if I try to define what is static, unchangeable, immutable in Slavic from the early Middle Ages or from Common Indo-European until the present. This is a static problem but at the same time a diachronic one.

Let us tackle the dynamic problems. I'll use as an example a change I observed from my childhood: there has occurred a certain salient change in the vowel pattern of contemporary Standard Russian. In the unstressed, especially pre-tonic position, the two phonemes /e/ and /i/ were distinguished by our grandparents' generation in Moscow. In the parlance of our and the younger generation, these two phonemes merged in one /i/. For the intermediate generation, that of our parents, this distinction is optional. What does this mean? It means:

the intermediate generation has a code that contains this distinction. When discrimination is needed to avoid ambiguities or to make speech particularly clear, both phonemes are distinguished in pronunciation. But in a slurring, slovenly, so to speak elliptic style, this distinction, along with certain others, may be omitted: speech becomes less explicit. Thus, for a certain time, both the starting point and the finish of a mutation appear to coexist as two stylistic layers and, moreover, when the time factor enters into such a system of symbolic values as language, it becomes a symbol itself and may be used as a stylistic means. For instance, when we speak in a more conservative way, we use the more archaic forms. In Moscow Russian, the generation of our parents did not use the distinction between unstressed /e/ and /i/ in familiar talk: rather the newer fashion of fusing both phonemes was followed to produce the impression of being younger than one really was. Suppose that there is even one generation that always makes this distinction and the succeeding one that doesn't make it at all. But it can never happen that only one generation exists and that the whole preceding generation dies on one and the same day. Thus both patterns must for a time co-exist, and usually there is some intercourse between the two generations, and the receiver of one of them is accustomed to recode the message from a sender of the other generation. Thus a change is, at its beginning, a synchronic fact and, insofar as we don't wish to oversimplify, the synchronic analysis must encompass linguistic changes, and, vice versa, the linguistic changes may be comprehended only in the light of synchronic analysis.

One problem becomes particularly pressing for structural linguistics. We haven't discussed this question here but it must be raised at one of the nearest Conferences—it is the problem that was alluded to so much in this country and abroad and which is still at the laboratory stage. I mean the problem of linguistic typology—the typology of patterns and the universal laws that underly this typology. What elements may occur together and what elements may not? What elements necessarily occur together? Which element B implies which element A and what elements don't imply each other? Which elements imply the absence of the other?

The discussion introduced by Osgood touching upon aphasia and the adjoined problem of children's language brings us directly up against the general laws of patterning. When I broach these problems, there inevitably appears a skeptical opponent who says: but we don't know all languages, so how can you tell that a certain structural phenomenon doesn't occur? Very well, but we know a high enough number of languages in order to say that if we subsequently find one exception to the supposed law, it will be a mere fraction of one per cent and the law will preserve the value of a weighty statistical statement with the probability less than one but still very close to one. Furthermore, there are laws of patterning where it is highly improbable that even solitary deviations will be found. Languages that don't admit vowels at word-beginnings are not rare but I doubt the existence of languages that don't admit initial consonants.

If there are general laws underlying the various phonemic and grammatical systems, we will hardly find general laws of linguistic changes. There will only

be observations on tendencies, statements on the higher or lower probability of variegated changes. For the possibility of a change, the only limitation is that there are no changes achieving a state that would contradict the general structural laws. This higher validity of static versus dynamic laws is not confined to linguistics. The developments of modern sciences records a similar conclusion in different fields. Thus we learned—I quote—that quantum mechanics is morphically deterministic, whereas the temporal processes, the transition between stationary states, are governed by statistical laws: in comparison with classical mechanics, quantum mechanics gains in morphic determinism what it loses in temporal determinism. To those who are easily frightened by far-reaching analogies I retort that I, too, dislike dangerous analogies, but I like analogies that are to me fruitful. And whether such interdisciplinary analogies are dangerous or fruitful the future will show.

Finally, one of the most symptomatic features of this Conference was that we lengthily and passionately discussed the questions of meaning. There were speakers who stated: a few years ago it would have been hardly possible. Well, that they were not discussed before also proved useful. Deliberations have their own agenda. One cannot discuss everything at one time. There are still those who say that questions of meaning have no meaning for them, but when they say 'no meaning' they either know what it means and *eo ipso* the question of meaning becomes meaningful, or they don't know, and then their formula becomes meaningless.

I think Smith's expression, 'differential meaning', is excellent. I should only like to add that any linguistic meaning is differential. Linguistic meanings are differential in the same sense that phonemes are differential sound units. A linguist knows that speech-sounds present, besides phonemes, contextual and optional, situational variants (or, under other labels, 'allophones' and 'meta-phones'). Correspondingly, on the semantic level we observed contextual meanings and situational meanings. But variations cannot be acknowledged without the existence of invariants. On the level of meaning as well as of sound, the problem of the invariants is pivotal for the analysis of a given language at a given stage. These invariants, puzzling for the cryptanalysts, are familiar to the habitual decoder who listens to a new context and nevertheless knows beforehand what its words mean, granted he belongs to the same speech community and is not a pathological case. The normal decoder recognizes the sound tokens through the phonemes, and the meanings of words and morphemes in the given message through the word-designs and morpheme designs in his code. If you, however, dislike the word 'meaning' because it is too ambiguous, then let us simply deal with semantic invariants, not less important for linguistic analysis than the phonemic invariants.

Smith, who has the rare gift of very concrete exemplification and presentation, and operates with 'differential meaning' as tangibly as with his rich uncle in the charming story he told us, said that we must find out whether the meaning is the same or different. He certainly realizes as well as we that it's easier to proclaim the principle of *Sameness* and *Otherness* than to resolve whether actually two

Sign-Events implement the same *Design*, or whether the two *Tokens* are to be assigned to two different *Sign-Types*. Identification and differentiation are but two sides of one and the same problem, the main problem of the whole of linguistics on both its levels—*signans* and *signatum*, to use the good old terms of St. Augustine, or ‘expression’ and ‘content’ as Hjelmslev christens them in his glossematic life work. This problem of identification and differentiation both on the level of ‘expression’ and ‘content’ is for us linguists an intrinsic linguistic matter.

It is true that some theoreticians claim that syntax deals with the relation between signs, and semantics with the relation between signs and things. Let us, within the frame of synchronic linguistics, examine: what is the difference between syntax and semantics? Language entails two axes. Syntax is concerned with the axis of concatenation, semantics with the axis of substitution. If, for instance, I say, “the father has one son,” then the relations between “the,” “father,” “has,” “one” and “son” are relations within the sequence; they are syntactic relations. If I compare the contexts—“the father has one son,” “the mother has one son,” “the father has one daughter,” “the father has two sons,” I substitute certain signs for others, and the semantic relations we deal with are no less linguistic than the syntactic relations. Concatenation implies substitution.

When I insist on the intrinsically linguistic character of semantics, is this novel? No, it had been very clearly said; only things that had been very clearly said are very often totally forgotten. It had been insistently stated from 1867 by Peirce who, I repeat, must be regarded as the genuine and bold forerunner of structural linguistics. As he said, the sign, in order to be understood, and the linguistic sign in particular, requires not only the two participants of the given speech event but needs, moreover, an ‘interpretant.’ According to Peirce, the function of such an interpretant is performed by another sign or set of signs that occur together with a given sign, or might occur instead of it. Here is the basis for our further deliberations on linguistic operations with meanings, and I’m sure they will be our main concern in the coming future. No doubt we will argue—argue about terminology, technical devices, some facets of theory—but the milestones are in view.

We learn still more how to incorporate grammatical meanings into structural analysis as the vivid discussion lead by our Yale friends, Lounsbury and Wells revealed. But even in interpreting the less schematically patterned lexical meanings, we may and must remain within the framework of linguistic methodology. Even were we to cling to the study of various contexts and confine this study to distributional analysis, lexical meaning still enters within its scope: a reversible equation sentence—A is B, B is A—is also a context; and one of the most illuminating of Peirce’s theses propounds that the meaning of a sign is the sign it must be translated into. When I read in today’s newspaper: “OPA permits pork price rises,” I personally don’t know what OPA is, but I do know the meaning of the words—“permits,” “price,” “rises” and “pork.” How may pork be defined from the linguistic point of view? “Pork is pig meat used as food.” Such an equational context is perfectly acceptable for the speech community, as well

as the reverse statement that pig meat used as food is called "pork." The statement informs us, as a matter of fact, only about the lexical meaning of the word "pork" in English. There are different ways of interpreting the word "pork" in other signs. We used a circumlocution, and we always may: as Peirce incisively defined the main structural principle of language, any sign translates itself into another sign in which it is more fully developed. Instead of an intralingual method, we may use an interlingual way of interpretation by translating the word "pork" into another language. The method would be intersemiotic if we would resort to a non-linguistic, for instance, to a pictorial sign. But in all these cases we substitute signs for signs. Then what about a direct relation between sign and thing?

To the very interesting discussion on the problem of pointing in the paper distributed by Harris and Voegelin, may I contribute a few remarks. Suppose I want to explain to a unilingual Indian what Chesterfield is and I point to a package of cigarettes. What can the Indian conclude? He doesn't know whether I mean this package in particular, or a package in general, one cigarette or many, a certain brand or cigarettes in general, or, still more generally, something to smoke, or, universally, any agreeable thing. He doesn't know, moreover, whether I'm simply showing, giving, selling, or prohibiting the cigarettes to him. He will gather what Chesterfield is and what it is not only if he masters a series of other linguistic signs which will serve as interpretant of the sign under discussion.

In Gulliver's Travels, do you remember the sage of Balnibarbi who decided "that since words are only names for things, it would be more convenient for all men to carry about them such things as were necessary to express the particular business they are to discourse on"? There proved to be, however, an inconvenience, pointed out by Swift, who was as clever in satire as in the science of communication: "if a man's business be very great, and of various kinds, he must be obliged in proportion to carry a greater bundle of things upon his back" and he risks sinking under their weight. It would be difficult to converse in things about 'a whale', even more embarrassing about 'whales', and hardly possible to communicate about 'all whales' or 'absent whales'. Even if miraculously succeeding to collect all the whales of the world, how to convey in things that they are indeed all?

As symbolic logic has persistently reminded us, 'linguistic meanings', constituted by the pattern of analytic relationship of one expression to other expressions, do not presuppose presented things. Linguists, on the contrary, did their utmost to exclude meaning and any appeal to meaning from linguistics. Thus, meaning remains a No Man's land. This game of Give-away must end. For years and decades we have fought for the annexation of speech-sounds to linguistics, and thereby established phonemics. Now we face a second front—the task of incorporating linguistic meaning into the science of language.

I realize that my remarks on pending problems are as fragmentary as a movie prevue, but you will understand them if Peirce is right in stating that any sign may be translated into another more explicit sign.

CHAPTER THREE

C. F. Voegelin and Thomas A. Sebeok

0. The place of language in the study of man must be focal to any discussion of language and culture, and it is therefore well to call attention at the outset to the shift from a tripartite to a bifurcate division of curricula in anthropology departments.

When Franz Boas was setting the standards for graduate work in American anthropology, it was generally realized that to qualify as a professional anthropologist one had to study—as coordinate systems—race, language, and culture. Not all students were equally interested in all three, and thus the compulsive study of a system which one found personally incompatible might lead to certain negative attitudes. Cole tells in his reminiscences¹ how Sapir hated the enforced study of physical anthropology when they were graduate students together at Columbia; how later, when they were colleagues together at Chicago, Sapir's obligatory knowledge allowed him to help plan a well rounded curriculum for a department which has done distinguished work in physical anthropology ever since. Still later, after Sapir had left Chicago for Yale, however, he was known to question whether physical anthropology really belonged in a department of anthropology—and, indeed, it has not yet become part of the department's work at Yale. Boas was likewise adamant when Cole tried to avoid linguistics in his graduate training; yet Cole did not reject linguistics when he had the power to do so as chairman of the department at Chicago. The curriculum at Chicago was the first, nevertheless, to show a bifurcated division of the systems within anthropology: biology versus the other half of the study of man, "the other half" including both culture and language. This immediately put the study of language in a less demanding or compulsive position; this bifurcated curricular arrangement at Chicago has significantly preceded the as-though theoretical and certainly explicit statement, so often encountered recently, that language is part of culture. But the shift from Boas' tripartite division to the University of Chicago's bifurcate division was a matter of curricular strategy and not of disinterested theory.

From the point of view of academic strategy, both pure linguists (those without specific training in cultural anthropology) and the sociological kind of cultural anthropologists are advantaged by the shift from the tripartite division of anthropology (race-language-culture) to the bifurcate division (human biology vs. culture including language). The latter provides a perfectly legitimate basis for the pure linguist's double professional association: (1) in linguistics (by virtue of his knowledge of modern linguistic analysis); and, (2) in cultural anthropology (through the logic of formal definitions). Conversely, it provides an equally legitimate basis for *accepting* phonemes and morphemes as the first strictly

¹ Fay Cooper-Cole, *Eminent Personalities of the Half Century*, AA 54. 157-67 (1952).

defined unit types in culture, while *rejecting* the need for culturalists to learn how to phonemicize or to morphemicize.²

The study of language as a function of something outside of the structure has been called metalinguistics, ethnolinguistics, psycholinguistics, and exolinguistics. Choice of the term metalinguistics is most often criticized as introducing an ambiguity since, in philosophical usage, it refers to the study of that language which we employ to talk about another language. As used by Trager and others, metalinguistics has come to refer principally to the theory that perceptions are categorized by a given language—a theory which the 1951 SSRC seminar of psychologists and linguists chose to designate as *Weltanschauung*.³

Anthropologists discuss this theory as a part of ethnolinguistics, as one aspect of the language-and-culture problem. Hoijer ascribed to Sapir the role of effective inventor, but for extensive documentation he turned to Whorf's Hopi data, to Lee's Wintu, and to his own illustrations from Navaho and other Apachean languages.⁴ Sporadic examples from still other languages are far from scarce in the literature—and their authorship precedes Sapir. Very likely, Boas introduced the theory in American scholarship. In the European literature, the notion is anything but new, which, of course, is not to say that the principles involved have been satisfactorily formulated or systematically exploited. Perhaps the most interesting recent elaboration is Fritz Mauthner's, in his three volume *Kritik der Sprache*, from which Wells, in his introduction, took the apt citation which might as well have come from Whorf: "Hätte Aristoteles Chinesisch oder Dakotaisch gesprochen, er hätte zu einer ganz anderen Logik gelangen müssen, oder doch zu einer ganz anderen Kategorienlehre."⁵

Lexical examples bearing on the theory are frequently given by students of Boas, as Jules Henry for the Kaingang,⁶ and by anthropologists relatively uninfluenced by Boas, as Gorer for the Lepcha.⁷ Both the Kaingang and the Lepcha examples have to do with the curious categorization of perception which *anger* or *angry* takes in these languages. On the lips of the Kaingang, *anger* is categorized much like *murder* in English—to say *I am angry at you* will cause you to be as apprehensive as if I had said *I will murder you*—and maybe this is what the

² C. F. Voegelin, A "Testing Frame" for Language and Culture, AA 52. 432-34 (1950); Culture, Language and the Human Organism, SJA 7.357-73 (1951); *per contra*, William E. Bittle, Language and Culture: A Comment on Voegelin's View (SJA 8.466-71), concluding that "The iconoclasticisms of modern linguistics should not be allowed to so thoroughly sweep away the notion of 'correct language' as to ignore its existence on the very real level of human behavior." (The important works of C. C. Fries document what effects talking about 'correct language' has had on the development of the English language—what zero effects!)

³ John B. Carroll, Frederick B. Agard, Stanely S. Newman, Charles E. Osgood, Thomas A. Sebeok and Richard L. Solomon, Report and Recommendations of the Interdisciplinary Summer Seminar in Psychology and Linguistics (1951).

⁴ Harry Hoijer, in A. L. Kroeber, *Anthropology Today*, 554-73 (1953).

⁵ Fritz Mauthner, *Neue Beiträge zu Einer Kritik der Sprache*,² 3.4 (1924).

⁶ Jules Henry, A Method for Learning to Talk Primitive Languages, AA 42.635-41 (1940).

⁷ Geoffrey Gorer, *Himalayan Village* (1938).

Kaingang really says. Among the culturally less aggressive Lepcha, *anger* is categorized much as we use *aroused* in English. Let us substitute *aroused* for Gorer's translation of *angry* in the following passage:

"There is no specific word for 'jealousy' in Lepcha. When I presented hypothetical jealous situations to Lepchas and asked them what their feelings would be, the greater number say they would be angry [aroused]; but this word, *sak-lyak*, does not carry very strong emotional connotations; it is the word used by parents if their children are naughty, or by workmen if they come across an unexpected difficulty in their work. It is also the most general euphemism for a sexual erection; I cannot untangle the associations which have produced this metaphor, but there is no question that the word is used in both senses; more than once I have heard a workman say when, for example, cords have got tangled 'I am angry [aroused] as a standing penis.'"⁸

A parallel kind of lexical lack could be matched from Tübatulabal where the word *təwə* has been recorded with a range of meaning comparable to English *good*; in eliciting *bad* the informant was asked to characterize the worst possible crime: *incest* was described as *haəš təwə not good*. The lack of a single morpheme for *bad* or for *evil* does not mean that the Tübatulabal does not perceive what we categorize in a single word *bad* or *evil*; just as *jealousy* may well be perceived among the Lepcha though without such a brief morphemic tag.

Morphemic absences seem not profitable as a subject for sustained research interest; but it is almost impossible to treat the Weltanschauung problem in any way except by including a consideration of morphemic absences.

Some people move away, and time always moves on (at least in English) but the language of the old home, of the old culture, persists. This is the survival theory, a special aspect of the Weltanschauung theory in reverse. Thus such languages as those whose kinship term for *father* is also applied to various relatives of father, as *father's brother*, and so on—in short, classificatory fathers—such languages have been taken as incorporating the survival of former customs, as reflecting in a present language what was actualized in a past culture. Classificatory fathers suggest a former stage of group marriage when Ego could not tell precisely who his biological father might be but who solved the problem by designating all males who had sexual access to his mother as his sociological fathers; so today, according to the survival theory, the putative father is reduced to one individual since the custom of group marriage has become extinct but the old Weltanschauung habit in reverse persists. It persists in language because the rate of change in language is much, much slower than the rate of change in culture.

Instead of persistence in the single system, let us now consider the problem of relating one system to another—linguistic acculturation, borrowing, and bilingualism.

Those of us who speak none other than our mother tongue are monolinguals. In certain entire communities (as in Switzerland) and in certain barrios (as in

⁸ *Ibid.* 162.

towns along the Southwest-Mexican border) almost everyone has two mother tongues. This kind of *community bilingualism* differs from *memory bilingualism* where, for example, a Hungarian leaves Budapest to live in Hollywood (speaking English but remembering Hungarian); or where a Navaho veteran reverts to the ways of his grandparents (remembering English but speaking only Navaho). In *memory bilingualism*, there is (1) some forgetting of the language not spoken, (2) ample opportunity for either language to influence the other, and (3) hardly ever complete learning of the second language, even if the first language is completely abandoned.

The new value studies, as exemplified by Vogt's Navaho Veterans,⁹ promise to make an important distinction between biculturals who have interiorized the second culture and those who have merely borrowed the external forms of the second culture; to be analogous to our *community bilingual* the bicultural individual should interiorize the values of *both* cultures. (This would lead to contradictions in the context of Vogt's bicultural Navaho veterans.)

In participant observation, linguist and culturalist are closer—e.g., when George Herzog plays drum signals on a Jabo drum. This kind of participation is probably rare in ethnography, but then it is also rare for the linguist to command any fluency in the language he is analyzing. Memory ethnography can mean that your informant remembers what someone in a preceding generation told him rather than what he experienced; one can, then, investigate culture by hearsay. But we cannot investigate language by hearsay; if, in an American Indian community, all informants speak English only, we say the aboriginal language is dead. A prerequisite for linguistic analysis is an actual sample, produced on the spot; we want only speakers who speak their native language—who then provide us with a standard of 'perfection' in that language.

It would be interesting to find an analogue in culture for the criterion of 'perfection' (as used by the linguist rather than by the native speakers). All adult non-defective native speakers speak their native language perfectly. Native speakers do not rate each other by virtue of knowing their own language. But in cultural performance much is rated by persons-in-the-culture. Thus, one hunter is cited as often returning empty handed, another hunter as generally good, and still another hunter as extraordinarily successful; so also, basket makers will rate the work of their colleagues; musical skill is likewise rated. Even individual skills in literary genre are rated (*qua* literary genre rather than *qua* knowing the language of the community)—someone is said to be good in oratory, or sometimes in a particular type of speech making, or good at memorizing ritual prayers, or at reciting genealogies. We do not distinguish ourselves from our fellows by learning to speak our native language 'perfectly.' Margaret Mead points out in a context somehow reminiscent of this that "a human natural language must be learnable and useable by human beings with varying and different types of discriminatory capacity."¹⁰

⁹ Evon Z. Vogt, Navaho Veterans, A Study of Changing Values, PMAAE-P 41, No. 1 (1951).

¹⁰ IJAL 18.258 (1952).

As actually practiced, one of the most fashionable types of investigation—and one of the least rewarding—is to see what happens linguistically in acculturational situations. What happens can be stated very simply; in linguistic circles it is called ‘borrowing.’ Beals has wryly observed that ‘few theoretical orientations’ have been produced in such studies. This theoretical poverty is hard to explain. It does seem to be true that, as one approaches interesting explanations one moves from *borrowing*, to some such precondition for borrowing as *memory bilingualism*; or to samenesses or differences in structure between the model language and the borrowing language. This relativism in linguistic structure probably in combination with the as-yet-little-understood-relativism in cultural relations may be behind the observable distinction between hospitable languages which welcome borrowing and inhospitable languages which do not. That the two factors here considered are independent variables is suggested by historical layers of borrowing when one model language is replaced by another model language vis-a-vis the same borrowing language—and/or when the same language serves as a more attractive model in one historical period than in another.

Linguists interested in both the structure and history of a particular language are apt to arrange their total data in such a way as to present their favorite as a uniquely borrowing language (abbreviated as BL)—i.e. they present their BL as constantly indebted in various ways to various donors, each of which serves their favorite as a model language (abbreviated as ML). Besides four types of loans in terms of similarity between BL and ML, Haugen notes¹¹ a possible kind of creation in a BL, as Pima, in which something from the culture (rather than from the language) of the ML—such as the physical presence of an *elephant* in a circus—will stimulate Pima speakers to create a descriptive compound, literally ‘wrinkled buttocks’. Here Pima unquestionably creates a new word for *elephant* but not *qua* borrowing language (because a BL needs a ML from which to borrow); it is safe to say that neither Spanish nor English has supplied the model for ‘wrinkled buttocks’. Here, rather, Pima exhibits a primitive sort of creativeness—primitive in the sense that some first language at some early time had to create new words in one way or another without benefit of another language to use as its model.

Borrowing is attested only when you have an explicit model language. Nevertheless, postulated borrowing (without explicit ML) is often asserted. Kroeber’s formulation of ‘stimulus diffusion’ has to do with a kind of cultural creation which may be suspected of having an indebtedness to a donor even though explicit evidence for the borrowing is entirely lacking or else is something less than statistically convincing. The American analogue of Trubetzkoy’s *Sprachbünde*, namely Boas’ linguistic area, is based on evidence which is not susceptible to being arranged in terms of Haugen’s four neat classes of borrowing—not so susceptible because Boas was unable to distinguish between the BL and the ML in his linguistic areas. Boas found only samenesses within an area; he seems to have assumed diffusional influence as the only way to account for such samenesses. He must have been led to this assumption by a parallel interpretation—

¹¹ Einar Haugen, *The Analysis of Linguistic Borrowing*, Lg 26.210–31 (1950).

that each different culture area likewise shows samenesses of culture traits which serve as the basis for setting up each particular culture area within which the samenesses have diffused. Boas' linguistic areas for North America failed to receive sustained attention from linguists—perhaps because they failed to correlate with Wissler's culture areas. Though actually not practised, one of the most promising kinds of research—full of promise for contributing to the language-and-culture problem—would be to see what actually does happen linguistically in a homogeneous culture area in which the languages are diverse, numerous, and without common ancestry.

For answering questions of this order, we can usually abstract relevant information from published accounts of both cultural and linguistic descriptions. In the past, field work has seemed appropriate for eliciting information on either basic ethnography or else on the grammar, rather than on any mixture of the two. Work in the library attempted to relate the two. And of course, most questions of general theoretical consideration begin in the study; thereafter, questions are elevated to a fixed viewpoint, sometimes even to a to-be-espoused-or-denied viewpoint.

Whether regarded as mere questions or as partisan viewpoints, the briefer (as well as the longer) way of stating the relationship between language and [the rest of] culture is arguable, as we know now that the issues have been stated. Two platforms have been provided in 1952—the Wenner-Gren Conference in New York,^{11b} and the Conference of Anthropologists and Linguists in Indiana—and adequate space in a half dozen scientific journals in the last half dozen years has been provided for that favorite activity of middle-of-the-century American anthropologists: 'sharpening concepts'. In their summary of the published literature devoted to this general discussion over the last 40 years (1911–1951), Kroeber and Kluckhohn ask whether part of the problem may not 'be unanswerable until the question is reformulated'.^{11c} As a final desideratum, besides sharpening concepts and reformulating questions, we propose discussion facing less in the direction of 'viewpoint' and more in the direction of 'notes and queries'—a humble asking of questions (old and new) to be tested directly in the rough and tumble of field work designed especially to elicit relevant information on problems concerning which information now available (incidentally) in ethnographies and grammars is—in one sense or another—insufficient.

1. Language and Culture

1.1.1. Introduction (Smith). Smith stated, first, that language is learned and shared behavior, and, as such, most people define it as part of culture; second, that language is systematic, and the system can be determined—it is the system through which, to a very large degree, all the other systems of culture are reflected and transmitted; and third, linguistics had made more progress than any other

^{11b} Sol Tax, Loren C. Eiseley, Irving Rouse, C. F. Voegelin, *eds.*, *An Appraisal of Anthropology Today* (University of Chicago Press, 1953).

^{11c} A. L. Kroeber and Clyde Kluckhohn, *Culture, A critical review of concepts and definitions*. Peabody Museum of American Archeology and Ethnology, Harvard University Press 47.124 (1952).

social science in the description, tabulation, and presentation of its data, because of the formal, handleable nature of these. Linguists have gotten out their elements and understood, as Hockett put it, the relation of the "allo" to the "eme." The techniques developed by the linguist could well be adapted or adopted by the other social sciences. Smith added that, as a linguist, he would proceed from linguistic data out toward the rest of culture; obviously, others can work in toward language. Linguists have criticized other social scientists for not being well grounded in linguistics; other social scientists have criticized linguists for not moving beyond their preoccupation with microlinguistics to its relation to the rest of human behavior. In their interest in the area of metalinguistics, both meet on common ground and, it is hoped, this may result in their mutual correction and enrichment.

1.1.2. Discussion centered on the line of demarcation between linguistic and non-linguistic study, especially with regard to the levels of linguistic analysis presented by Smith, and on kinesics.

McQuown described three ways of seeing the relation between language and culture. If culture is all learned behavior, language obviously is part of it. If culture is that part of learned behavior which is transmitted through the medium of language, clearly language is not part of culture. A third way is to see language as a model for what goes on in culture. McQuown did not at all accept the "language-in-culture" phrase used by Smith, although a great part of culture is transmitted through language. One can conceive culture in terms of the media through which it is transmitted; these are not hard to define physiologically. One might try to evaluate them: language, as the most important vocal gesture, and so forth.

Smith presented the conceptual scheme developed at the Foreign Service Institute which divides linguistics into three main compartments: "prelinguistics" studies the physical and biological aspects of speech; "microlinguistics" deals with the analysis of linguistic systems; "metalinguistics" states the relations between the language and any other cultural system. Together, these constitute the whole field of "macrolinguistics." While in metalinguistics one deals with meaning, in microlinguistics one deals only with differential meaning, that is, whether utterances are the same or different. On this level, grammatical meaning is a combination of differential meaning and distributional facts, or internal meaning. Smith noted objections to the term "metalinguistics." He suggested seeing that field as having three parts. Metalinguistics is the consideration of the style and meaning of the microlinguistic system itself. Either "exolingistics" or "ethnolingistics" is the point by point comparison of the linguistic system with another system of the total culture. "Psycholingistics" includes the effect of the way in which language structures experience for the individual.

Chao pointed out that "metalanguage" is used for a language with which one discusses language, so that metalinguistics might suggest a language in which one talks about linguistics. Voegelin said that, although the Conference was supposed to be concerned with language-in-culture, the term "metalinguistics" includes no reference to culture of which it is supposed to be a part. Hockett stated that the third compartment is not supposed to subsume everything else in

culture outside of microlinguistics, but supposed to subsume only the interrelations with the rest of culture. Hjelmslev requested a metalinguistic analysis of the English intonation morphemes which Smith had demonstrated earlier so as to make clear the difference between differential meaning and the meaning in general of the intonation patterns. Smith replied that the intonation patterns had no meaning in general, no encapsulated meaning. He stressed the importance of this point. Hill said that the history of linguistics has been characterized by sharper and sharper divisions of levels, each stage marking an advance. He gave an example of the sort of methodological error which Smith's separation of microlinguistic and metalinguistic levels would avoid. In dealing with literature, he proposed the same three levels but with the difference that microlinguistics would be preliterate, presumably a part not yet fully differentiated. Shimkin considered the problem largely one of differentiating the anthropologists' and the linguists' standpoint. From an anthropologist's standpoint, he considered the important boundary that where linguistic terms can no longer be used and where the direct relationship must be made with other forms of cultural content; the divisions within linguistics itself are not so important. What is important is the extent to which linguistic data can refine the problems with which the anthropologist deals. In historical problems, comparative philology is one of the most powerful and least used tools for the study of cultural relationships. He suggested the boundary be drawn somewhat further out than the "micro-meta" boundary. Birdwhistell¹² remarked that this division of levels would prevent reading in one's own cultural responses when applying psychological tests. In response to a question from French, he said that the linguistically untutored investigator testing a Kutenai and an American child might record the vocal response but ignore the suprasegmental morphemes. He might consider them "natural," yet the differential in response might be carried in these intonation morphemes; it might be the thing that makes a Kutenai response different. Smith stated that the major part of the communication situation is summed up by the combination of the linguistic system on all its levels, and the kinesic system. Chao observed that perhaps there are parts of communication which are not to be described even in metalinguistics but which are just "meta-".

Birdwhistell then explained the notational system of kinesics,¹³ and followed it by a lively demonstration of the method, presenting a mother-child situation and its analysis.

Bar-Hillel challenged the clearcutness of the distinction between microlinguistic and metalinguistic levels, and of the distinction between linguistic and kinesic systems. Smith replied that (1) microlinguistics stops with differential meaning, and that (2) vocal gesture and body gesture are analyzed separately in order to handle their complexity. Kinesic phenomena can reinforce or negate what is analyzed on the linguistic level, just as voice qualifiers can reinforce or negate what is analyzed on the linguistic level. By keeping levels apart, one can make point-by-point correspondence between them. When these things with differentia

¹² Ray L. Birdwhistell, University of Louisville.

¹³ *Id.*, Introduction to Kinesics (1952).

meaning are put together, one can then get an idea of what they mean as a totality in the cultural context. Voegelin defended the position that the line between linguistics and non-linguistics could be clearly drawn, and Ray agreed. Horowitz¹⁴ mentioned the chance that the informant cannot say that something has differential meaning. If one can get clearcut classification of something as to same or different, there is justification for setting it off as a separate level or system; if not, not. Smith countered that one must develop one's techniques for determining sameness and difference; one may have to put the material into the right context for the informant. The line between linguistics and kinesics, he stated, can be drawn by definition. Casagrande remarked that some material noted in the kinesic example might be meaningful for the analyst, but not part of the significant communication between mother and child. Birdwhistell replied that (1) until each part had been tested repetitively, one did not know which had significance; (2) it was known that body tension, below the level of awareness, could communicate. Lees¹⁵ pointed out that gesture, which must be visible, and speech, which need only be audible, can be catalogued separately then compared to see if they structure together. Lévi-Strauss asked how one can distinguish two kinds of gesture, that resulting from actual body strain and that which is cultural. Birdwhistell replied that more knowledge is needed from psychologists and physiologists before one can say. The study of a series of individuals avoids getting an idiokinesic system. French questioned whether there is as much conscious teaching and correction of gesture as of language and other parts of culture. Birdwhistell said the answer was not yet known, but gave examples of over-correction of gesture behavior. Hockett concluded with the statement that kinesics was a valuable new avenue of study and that such questions as universal kinds of gesture must wait for further inductive work.

1.2.1. Introduction (Sommerfelt). Sommerfelt asked: can we establish correlations or interdependence between the system of communication and other parts of the social system? Lexicon obviously reflects society to a great extent. The great problem involves grammar and phonemics. Comparison of grammar to social system so far has had one of two drawbacks: either other languages were viewed through European eyes or comparison was limited to complicated and similar languages such as French and German. One must choose very different linguistic structures for the comparison: structures which are archaic or the oldest type now available for study. He stressed the need for further research and for reliable information about the fundamental categories, the formal structure of a great many different languages, before progress could be made on these problems. One should not expect immediate correlation between linguistic form and other social facts; among complicating factors is the conservative character of language. For correlation with phonemic systems, more material is also needed. Jakobson's work on children's speech seems the most fruitful approach. Two UNESCO projects are relevant, one for a structural atlas of the languages of the world, the other for a conference which will relate the results of descriptive studies

¹⁴ Arnold E. Horowitz, Harvard University.

¹⁵ Robert B. Lees, University of Chicago.

now underway to the social facts of the cultures where the languages are spoken, in order to solve the question of different "logics" among primitive groups. If linguistic form can be correlated with social facts, this proves its social origin. All linguistic change is of social origin, but this question of historical factors in linguistic change must be kept separate from the question of the correlation between linguistic categories and culture. The "innate form" of culture mentioned by Sapir might be comparable to language.

1.2.2. Discussion. Conservatism in language was urged. The discussion centered on the relative complexity of language, parts of language structures, and cultures.

Birdwhistell challenged the statement that language was more conservative than other parts of culture, comparing it to the concept of cultural lag, which he termed a curse in social science. He asked if change could take place in the social order without a concomitant change in language. Sommerfelt replied that there would be some change, but it might be only in details. Householder interposed that Birdwhistell did not distinguish form from meaning, that his examples came from lexicon, not structure. Jakobson also stressed the difference between grammatical and phonemic patterns on the one hand, and vocabulary on the other. The difference exists because (1) grammatical and phonemic structure is learned very early and remains essentially unchanged through life so that the attitude of the speaking community toward it is very different from the attitude toward vocabulary; and (2) there can be much greater conservatism in grammatical and phonemic structure than in the rest of language and in social structure. Language, however, can be less conservative than the rest of culture; immigrants may lose their native language but retain culinary arts or attitudes toward the body.

1.2.2.1. Relative complexity. Jakobson stated that a language with a large geographical extension never has as complicated a repertory of phonemes and distinctive features as sometimes occurs in the languages of small groups. Hill pointed out that the history of English shows an increase in number of phonemes, not a decrease, while English was expanding geographically. Sommerfelt replied that in English it was not a question of many phonemes or a complicated system. Bar-Hillel said the number of phonemes was not the only important fact. He asked if linguists had other than an intuitive basis for speaking of the greater complexities of a language. Sommerfelt answered that "complicated language" was short for complicated grammatical structure—a greater number of patterns vs. a smaller number of patterns. Bar-Hillel said that this was with respect to particular features only; and he could not imagine a way of weighting all the features in a language so as to compare two languages as wholes. Smith pointed out that the phonology must be completed on all levels before statements of complexity and simplicity can be made. The Latin vocative has a special case ending, but the suprasegmental phonemes used in the English vocative are at least as complicated. Hockett stated that the total complexity will be roughly constant in all languages, and must be. He suggested that one might be able to measure morphophonemic complexity if text frequency is considered; and that perhaps a language spoken over a large area cannot maintain the same degree of morpho-

phonemic complexity. Birdwhistell asked what specific criteria or counts have been used to determine cross-cultural complexity for any item. Jakobson called it ridiculous to talk about the complexity of language in general. One can speak only of the complexity of different levels or particular features. He stressed the difference in result if one studies texts, messages, or code. Voegelin suggested that if a closed corpus, or single text, were examined, French might prove superior in some way to Hottentot, but that given an open corpus, any two languages would prove equal in their capacity for expression. He asked the culturalists present to state how they regard culture in respect to complexity. Ray stated that whereas cultures were once regarded as all equal, whether highly developed or not, today the answer is that they are not equal; but, as Voegelin said of the change in the kind of assertions made by linguists, there has been little new in the way of proof. Osgood clarified the use of the term "complexity." He distinguished the items from the arrangement of items or types of dependency relations. He considered the real problem to be how many alternatives extended at points of choice, suggesting that one language might have many phonemes but few choices at given points in the stream of speech, while another language might have few phonemes but many choices at given points. Ray commented that one should suspect greater complexity in a culture with a greater number of elements, simply because of the mathematically greater number of possible relationships. Gleason¹⁶ interposed that when there is talk of items which so-called primitive languages and cultures do not have, it may be because they simply have not been found. There are few languages for which we have a complete phonetic inventory; in general we don't know what we have in linguistics, and anthropology is worse off. Ray agreed and illustrated the point from cultural anthropology: the Thompson River Indians, once thought one of the best described tribes, were found to be known for only one out of five elements on the culture element survey. A simple count of available data would have had to rate them less complex than many simpler tribes. Hjelmslev argued that a statistical basis for complexity was dangerous because count depends almost exclusively on the method chosen. It is safer to say that phonemes and morphemes do not exist in a "realist" sense, as the term is used in technical philosophy, but that they are tools which we use. It is much more important to consider distributional features and the rules governing the use of morphemes and phonemes. It is very dangerous to state general correlations between linguistic systems and given social structures, and safer not to compare form with culture but meaning with culture. Form may persist while meaning changes; and where meaning reflects culture, it often reflects a much older culture. Outside of this, correlation between linguistic systems and culture either is impossible or gives no results. Ray agreed that a judgment of complexity has nothing to do with statistics. For cultural anthropology it has to do only with its unique methodology; but as for the general correlations between culture and linguistic systems, it should not be said that one cannot make them or should not make them.

¹⁶ Henry Allan Gleason, Jr., The Hartford Seminary Foundation.

2. Speech and Personality

2.1.1. Introduction (Chao). Chao presented five levels of speech behavior at which personality traits can be analyzed.¹⁷ The five levels are, (1) voice quality; (2) voice dynamics, including intonation, rhythm, continuity, and speed; (3) pronunciation; (4) vocabulary; (5) style. He stressed the problem of distinguishing between the individual and the social elements in such analysis. For personality, he posed three questions: "when" is the problem of attributing the determinants of personality in speech to inheritance at birth, the period of childhood, or the whole life experience; "where" points to the different speech-personality features in different situations; "which" points to the bilinguals, split-personalities and others whose differences in speech might be said to reflect different personalities.

2.1.2. Discussion. The main topics discussed were culture-personality studies, the concept of personality; and the concept of idiolect.

2.1.2.1. Culture-personality studies. Shimkin said the culture-personality work of some anthropologists has produced unreal abstractions about *the* personality of a culture, whereas there are important individual differences in even simple cultures. French responded that some of these statements should be called "real" abstractions since they designate things which make a difference for the people and their behavior. Shimkin cited the work of Newman and Mather¹⁸ which showed marked lexical and morphological changes among manic-depressives during treatment, regarding linguistic keys to personality factors. Osgood said that personality variables would enter where there was linguistically free variation. The one kind of feature which shows marked variation with the course of psychiatric treatment, he said, is not vocal characteristics, ratio between word-classes, and the like, but change in meaning. This indicates that one must look for the major effects of the relation between language and personality in semantic areas. Sommerfelt cited a case of purely phonetic features revealing personality. Shimkin emphasized the results of projective tests such as Rorschach and TAT. He considered these fairly good guides for cross-cultural work, giving perhaps eighty percent predictability. Lévi-Strauss said that the so-called culture-personality studies have discussed micro-cultural types of option, not personality; not culture and something else, but culture and a more limited part of culture. He called the results of Rorschach tests not only amazing but a scandal. Their results indicate they are not psychological tests at all, but sociological. Since no culture can integrate all the individuals it tries to integrate, mental disturbances and pathological cases occur, which must be approached as cultural phenomena. Personality is not a problem about the relation of psychology and anthropology, but a problem completely internal to cultural anthropology. Shimkin argued further that not just culture, not only what is learned, but the individual's total

¹⁷ These were based on Edward Sapir's Speech as a Personality Trait, *American Journal of Sociology* 32.892-905 (1927).

¹⁸ Stanley Newman and Vera G. Mather, Analysis of Spoken Language of Patients with Affective Disorders, *American Journal of Psychiatry* 94.913-42 (1938).

experience affects personality. Culture is one vector. He urged also that cultural modality not be lost sight of; statistical sampling of different groups may not reveal great differences, but the social ideals may differ greatly; and here the concept of modal personality is very useful. Osgood said psychologists, including clinicians, are increasingly dubious of projective tests. Rorschachs seem to get such good results because they are a double-projective test; the investigator uses the responses of the subject as an inkblot. While acknowledging some limitations, Shimkin defended projective tests as very promising.

2.1.2.2. The concept of personality. Lévi-Strauss's critique of culture-personality studies evoked argument over the nature of personality. Osgood restated Lévi-Strauss as asserting that personality is no more than the patterning or grouping of social roles. Lévi-Strauss replied he had only referred to what anthropologists have been studying under the label of personality. Osgood went on to attack this approach. If personality is equated with roles, this leads to positing the number of social groups to which an individual belongs as equal to his number of potential roles. But when one counts, one ends up with a number of groupings, of cultural roles, that equals the number of individuals in the culture. The pattern of potential roles of any individual is always unique. Lévi-Strauss replied that in any society there are a finite number of roles and these are what anthropologists have been studying, not personality from the infinite psychological level. French said that, after roles have been described, there still remains material in an individual's behavior of a different kind. Bar-Hillel asked whether behavior which was predictable because of an individual's membership in a certain class would be considered personality. He suggested that all behavior was not predictable by knowing an individual's membership in certain classes and that this remainder be regarded as personality. Jakobson emphasized the cultural distribution of stabilized types of roles. He agreed with Osgood that the optional linguistic elements are most susceptible to personality variation, but he deplored the tendency to overestimate the amount of free variation and the role of personality in folklore. In a Russian village, the folktale narrators choose from several possible styles which are completely culturally elaborated. Personality here is not a free combination of elements but an expression of various possible types. Chao had asked if it is valid to ascribe characteristics to personality regardless of how acquired. Given a person whose cultural origin was completely unknown, could any of his characteristics be said to be personality? Wells answered later in the discussion that if the culture is that in which an individual is like the rest of the community, and personality is that in which he is not, then Chao's question is answered. One cannot distinguish personality without knowing the cultural background with which it contrasts. Osgood stated that psychology has no integrated, consistent picture of personality to offer. He described three main points of view: (1) that of perceptual orientation, regarding personality as a system of ways in which people perceive; (2) that of the behaviorist, regarding personality as a system of habits; and (3) the integrative viewpoint, regarding personality as a bundle of distinctive traits. He suggested a synthesis in which the trait is taken as a channel or construct which renders equivalent a set of stimulus

situations and a set of behavior responses. This leads to the possibility of measurement in linguistics which he illustrated with a chart.

2.1.2.3. The concept of idiolect. The discussion of personality and culture-personality studies was interspersed with queries about idiolect. Barker asked for a definition of it from the linguists. Olmsted suggested it might be equivalent to the linguistic markers of personality. When French again asked for a definition, Chao answered that it was a rather new term, not generally used. It is generally limited to the microlinguistic study of language and would not include voice quality, style, speed, or some other personality factors in speech. Jakobson insisted on the primacy of the socialized aspect of speech, the dialogue rather than the idiolect, though not wanting to rule out the idiolect from study. Weinreich¹⁹ declared idiolect a makeshift concept enabling the linguist to describe the language of a group from one or a few individuals and call it complete. The description of the idiolect contains no information on how typical it is or what sector of the community is represented. Further, the idiolect does not even exhaust individual variation but may represent only the special situation between informant and linguist. Chao replied that in a large community one can fairly quickly select a typical speaker; that by the nature of linguistic data, one can assume a lot from one informant; that conditions often prevent working with more; as an act of modesty, the linguist says that such results represent only an idiolect.

2.2.1. Introduction (Shimkin). Shimkin presented the paper which he had prepared for the Conference on the use of proverb data to investigate attitudes and other psychodynamic features. His material was drawn out of proverb collections from three pre-revolutionary Russian communities. The experiment tried to codify, quantify, and interpret the material in the proverbs so as to reveal the pattern of social relations, the philosophical orientation, and the gestalt (logical and perceptual orientation) of the three communities. It was assumed the material could be related to Rorschach principles.

2.2.2. Discussion. The discussion centered on questions of procedure and questions of interpretation.

2.2.2.1. Questions of procedure. Wells first raised the question of data on the frequency of use of the proverbs. Casagrande and E. W. Voegelin raised the question of relating the proverbs to the situations in which they are used and the individuals who use them. Lévi-Strauss challenged Shimkin's approach, stating that all of the three groups should have been included in the study, rather than a sample of the collections; he felt that the study should not have emphasized the proverbs which were different in each group, but the differences among those which were similar. French categorized scientists as "similarity-stressers" and "difference-stressers" saying that which approach to use should depend on the problem and the method. C. F. Voegelin gave an example from Turkish of how one could predict an individual's personality and behavior from the types of proverbs used. Osgood said statistical analysis should have been carried further by use of a significance test. He suggested treating some sample on basis of em-

¹⁹ Uriel Weinreich, Columbia University.

pirical categories rather than Rorschach or gestalt-derived principles, then making a contingency analysis. Kiparsky asked why the study was limited to three collections. Shimkin explained that eighty operations were necessary for each proverb. Hill stressed the need to analyze the form of the proverb, since a required form limits the type of material which can be used. Jakobson mentioned that formal study of Russian proverbs showed enormous limitations.

2.2.2.2. Questions of interpretation. Jakobson stressed that proverbs in ideological content are frequently survivals of many different levels. Kiparsky, agreeing, called proverbs a folkloristic residuum. Shimkin said his paper had pointed out that proverbs are not an undistorted picture of society; he added that the distortions, such as stereotypes, may give valuable clues in themselves. French argued that if people use a proverb, it is because it has current value and meaning, whether or not it is old. Stereotypes about foreigners are important in showing in-group vs. out-group attitudes, even if the foreigners named in the proverb are no longer in contact with the people. Bar-Hillel considered it of little scientific value to know what proverbs exist for potential use but thought that knowledge of what proverbs are used in given situations might yield predictive statements. Osgood summed up much of the discussion, calling proverbs a storage bin of various historical depths suiting all occasions. The problem is one of selection, depending on a personality or need factor. A sample of what is used is not enough; one must get the relative frequency of usage. This might be a subtle way of getting at deep systems; the relative usage from one period to another might be a way of getting at changes in attitudes.

Weinreich asked if it should be concluded that Shimkin's type of material was not the most profitable and work should turn to material which is observed in living usage. Shimkin pointed out difficulties in obtaining material by participant observation. Also, one must quantify either on the basis of number of proverbs, on a theme or on the basis of frequency of use; but for the latter, how can one standardize the social situations. The nineteenth century collections were used in part because certain problems required data from that period. Bar-Hillel asked if the results made it possible to predict the social and psychological structure of a fourth Russian village from its proverbs. Shimkin replied that proverb data complements but does not replace ethnographic data. Bar-Hillel asked if, conversely, one could predict a certain statistical distribution of proverbs from ethnographic data. Shimkin answered, not on the basis of three samples. Olmsted suggested comparing results from analysis of collections and from observation. This might be done in Dahomey.

Most of the discussion was directed at Shimkin who said he was pleased that his own study had brought out such wide discussion of the value of proverb data.

3. Dialect Distance and Culture History

3.1.1. Introduction (Twaddell). Twaddell described two directions from which this topic could be approached: (1) human interaction across differences in the interacting systems, i.e., "talking across isoglosses"; (2) reconstruction from present-day evidence of an otherwise inaccessible state of affairs. Linguistic

reconstruction of this sort and cultural inference from the reconstructed forms are familiar and risky procedures.

The first direction is the problem of human interaction through codes which are not entirely isomorphic. Linguists play with three terms in this regard: "idiolect," "common core," and "overall pattern." "Idiolect" is a summary of the behavior of one human being making use of vocal signals over a fairly restricted period of his life. While it is a statistically sketchy corpus, there is considerable carryover from one idiolect to others in the community. When utterances are swiftly and with assurance identified despite idiolectical differences, they belong to a "common core." An "overall pattern" is a notational system general enough to provide for the material in all idiolects under consideration, but not so general as to include material not included in the idiolects. When the common core is very near the totality of the idiolects, they are extremely similar; when it approaches zero, we say there are dialect differences. A common core of gestures permits some communication without language. There is a critical size of the common core when it may be large enough for persons to attempt interaction but not large enough for this to be effective. When there is less than a complete common core, the mechanisms of communication reflect human ability to make good guesses on incomplete data. The listener is prepared to make choices between a finite repertory of probable messages in most situations. Such is the super-super redundancy of language in such situations that one needs only a few clues. One may say that the speaker puts out distinctive features identifying the message while the hearer perceives decisive stigmata which identify the message for him. What is a distinctive feature for the speaker need not be a decisive stigmata for the hearer, which is how we can talk across isoglosses.

As an example, Twaddell presented the differences which occur among seven dialects of West Germanic for bilabial phonemes in initial position. While dialect differences were greatest in this phonological domain even here, presumably, communication was possible because of an intermeshing of what were distinctive features for the speakers and decisive stigmata for the hearers.

3.1.2. Discussion. The discussion ranged over several subjects, taking up the West Germanic example, redundancy, bilingualism and switching code, dialect distance, and second language teaching. The three concepts of idiolect, common core, and overall pattern were interwoven.

Ray asked if the West Germanic peoples might not have been bilinguals who had learned the necessary features. Twaddell replied that the differences involved had remained for 1200 years; it was unlikely a bilingual situation would persist so long on such a knife-edge balance. Hockett said the distinction was not a real one. When communication occurs, the people have learned to understand one another, whether between idiolects or languages, and that which identifies a message for the hearer still need not be what the speaker sends. Osgood suggested a test: if it is not a bilingual situation, speakers would have no trouble the first time they heard each other; in a bilingual situation, they would have to learn a new code. Smith asked if Twaddell would put all seven dialects into an overall pattern. Twaddell said he thought this possible. Smith then said that an overall

pattern and a context of more than one word were enough to identify a message. Lees distinguished two kinds of interdialectal differences: (1) the replacement of a feature in one dialect by a corresponding feature in the other; and (2) a difference in distribution of the corresponding feature between the two dialects. The latter requires an overall pattern; the hearer needs other features to tell which word has been offered. Haas suggested that there might be considerable intelligibility between speakers of the dialects even if all three phonemic distinctions fell together. Twaddell replied that the specimen given was an extreme case of ability to establish congruence across boundaries; the stigmata involved were of the fifth, sixth, or seventh order; for far more conspicuous features in the speech were always present. If the speakers had had to depend on these three phonemic distinctions to identify messages, it would be an extreme case; even so, they could be sure of these relatively trivial phonemes. Voegelin mentioned the drum-signalling of the Jabo where in the context of war the only stigmata needed to identify a message are tones.

3.1.2.1. Redundancy. Osgood referred to research at Harvard and MIT on the deliberate experimental distortion of acoustical input in order to measure the effect on intelligibility. It is found to depend heavily on context. In familiar material, there can be eighty percent distortion, but with the individual phonemes there is extreme trouble. This suggests a different kind of explanation, if the stigmata are rather specific cues, in terms of redundancy. This should recognize that redundancy may be in terms of the behavioral context in which talking occurs. Twaddell clarified the concept of stigmata as those features which are decisive for identifying one message in a repertory possible in a given situation. Hockett stated that any sufficiently large percentage of the gross acoustic output, whatever features are involved, will be enough for communication under many circumstances. Often, enough is transmitted in the first fraction of a message, as in a familiar poem or proverb. If so, why do we ever bother to finish? There must be something in language besides communication. Lounsbury illustrated another kind of redundancy where morphophonemic differences which sound extreme to our ears are not perceived as different by native speakers of Cayuga.

3.1.2.2. Bilingualism. Jakobson said that the problem of switching code from one language to another was the problem of bilingualism. He referred to his personal experience as to how fast one can learn to translate from one code to another. The problem of overall pattern was much discussed in Russian linguistics and one of importance. He gave an example, however, of a Siberian language in which there are essential differences between the sound-patterns learned by men and by women. In such cases the notion of overall pattern might not be necessary. Twaddell considered Jakobson's points of extreme importance from the viewpoint of individual bilingualism, stating he would like the tools to classify patterns which are bilingual in terms of the individual autobiography and those which are part of a common core in messages. The example of communication between men and women with different sound-patterns was simply code-switching. He emphasized again the question: what are the characteristics of trans-isoglossic communication which are biographically classified in terms of the individual's

code-switching capacities and those which have to be linguistically specified in terms of common core. Haas suggested distinguishing a receiver-bilingual, who can operate as a receiver but not send, and a sender-bilingual, who can do both. She gave an example of sex differences in speech in which fathers correct the speech forms of daughters and mothers those of sons. French offered an explanation in terms of the difference between recognition and recall. When men use the women's language, they both recognize it and recall. But often, when hearing another language, people may recognize the messages, yet are unable to reproduce them, let alone recall them. Jakobson gave an instance in which men and women can pronounce features characteristic of others' speech, but refrain. There are cases in which people know how to pronounce forms of a neighboring dialect but do not. Weinreich said that the individual's code-switching capacity as described in a biography was almost a personality trait. Bilinguals differ greatly in their code switching capacity and in ability to learn a second or third language and to discriminate between them. He reported the results of tests with Welsh children, and his own experiments in Switzerland, which discovered marked differences between children whose responses to stimulus words in two languages showed preoccupation with the associations within the language and those whose responses showed preoccupation with translation. This shows a definite possibility of personality correlations. Lévi-Strauss stated that it was not a purely linguistic problem, for there is sharing of culture as well as of language. In culture, too, an overall pattern makes possible a switching of code. The discussion of differences between two dialects had been carried out as if in some circumstances the same things would be said, but this is not the case. Osgood strongly agreed, saying that the extent to which cultural traits match will be very important for the ease with which code-switching takes place. The discussion should be extended to the problem of meaning. Just as there is common core in structure, so there are important common cores and differences in meaning.

3.1.2.3. Dialect distance. Chao called attention to Voegelin and Harris' work on objective measures of intelligibility between dialects.²⁰ He asked whether one could compare lists of features in dialects, and compare these with the objective results of the intelligibility tests. Bar-Hillel asked why the discussion of common core was limited to dialects. Might there not be a sufficiently one-to-one, or many-to-one, switching code between two languages for understanding? Twaddell replied that the crucial fact was the existence of some common core whether on the morphological or phonological level. Smith asked if a workable trial differentiation between language and dialect would be that, if two systems can be gotten back into an overall pattern, one is dealing with a language, while, if it is not possible, one is dealing with different languages. Hockett said no.

3.1.2.4. Second-language learning. Myers²¹ inquired if the concept of common core might aid in the second-language teaching of English. If only a certain

²⁰ C. F. Voegelin and Zellig S. Harris, *Methods for Determining Intelligibility Among Dialects of Natural Languages*, *Proceedings of the American Philosophical Society* 95:322-29 (1951).

²¹ Colonel Adolph Myers, *Technical Cooperation Administration*.

facility for switching codes, or possession of a common core of pronunciation, were necessary, might it be possible to give up much of the time devoted to pronunciation, saving it for vocabulary and grammar? Might it be possible to know just how far one would have to go in establishing sufficient accuracy? Twaddell replied that he felt no time spent in bringing pronunciation as near perfection as possible is wasted. The learner would be so deficient in judging usage that he must be as perfect a sending and receiving machine as possible. Chao said his experience with Chinese showed that it pays to give a great deal of attention to pronunciation, since, because the number of phonemes is much smaller than the number of other elements, this one part of the language can be mastered in a short time, allowing a rapid pace after that. Smith said the pronunciation of English cannot be slighted; since English phonology is so complicated it becomes the main problem in teaching.

3.2.1. Introduction (Voegelin). Voegelin suggested a set of terms in culture to parallel the terms in linguistics which had been discussed during the afternoon.

3.2.2. Discussion. Discussion concentrated on equivalences between linguistic and cultural terms, following Voegelin's lead. The three linguistic terms were further analyzed, and some general comments on the status of linguistics and culture vis-à-vis each other were made.

3.2.2.1. The linguistic terms. Smith asked Hockett to explain the three linguistic terms as he had done in conversation before the session. Hockett defined "idiolect" as the individual's total repertory of speech habits over a short period of time. "Common core" is the sub-systems of the idiolects of people in a speech community which are isomorphic. "Overall pattern," operationally, is to take forms from a random group of speakers, act as if there were one speaker for all, and apply usual analytical techniques. This makes sense only if the idiolects involved have a common core. Individual speakers in their idiolects can be said to have a selection from the overall pattern. No idiolect, by definition, is more extensive than the overall pattern. Twaddell said that all three linguistic terms refer to lists of items. An "idiolect" is a repertory of the linguistic events emanating from, or shared in by, an individual speaker. "Common core" defined in terms of the idiolects it is to cover, is the repertory of those events which are similarly ordered in a number of criteria. "Overall pattern" is the repertory of all those events which can be compared of the speakers previously considered as having a common core. Idiolects are not guides but lists. Hockett stated that if each idiolect is taken as a mathematical system, the algebraic sum is the overall pattern, and the algebraic product is the common core. These terms do not refer to operations between languages; one can come close to defining a language as having a common core and overall pattern. Lounsbury distinguished talk about idiolects as repertoires, from talk about them as structures, analyses. With an idiolect as a repertoire, the common core would be the class product of the idiolects, and the overall pattern would be the class sum. But, if idiolects are talked about as analyses, it is no such simple thing as class product and class sum. Weinreich considered Lounsbury's point important for bilingualism and biculturalism. There is the problem of regarding the contact of idiolects and idiocultures as

structures rather than lists. One may describe phonemics in such a case either as a merged system or as separate coexistent systems; the latter is closer to the experience of the bilingual. Smith demonstrated the overall pattern of the vowel system of English. He had previously worked from idiolect to overall pattern; Hockett suggested common core. Ray said that, similarly, cultural anthropology had not worked with common core. The concept of culture utilized but did not distinguish common core and overall pattern. It may be a profitable new lead. Studies of culture certainly do not limit themselves to those things in which all participate. Hockett mentioned that Martinet had worked out a common core for French vowels under the label of archiphonemes.

3.2.2.2. Equivalent terms. French considered personality the most promising of those matched by Voegelin. In place of the others, he suggested common core and overall pattern on the cultural side, since there are no comparable terms at present in cultural anthropology. He gave an example of common core and overall pattern from Sahaptin tribes. Lévi-Strauss said that what Voegelin had put on the board did not make sense: the differences between individual speakers tell a great deal, not about personality, but about culture. Bidney²² said the difficulty in finding common terms was the different categories involved. Idiolect refers to something idiosyncratic, common core to types, and overall pattern to an aggregate of traits. If one sticks to the notion of pattern or type as in culture, one can construct a scheme in linguistics that parallels it. Close correspondences would be "real" for common core, "ideal" has no correspondence in linguistics as yet, and "culture-construct" for overall pattern. Ray said that, if the linguistic terms are lists, they cannot correlate with patterns in culture. The most similar term for idiolect would be idioculture, but "culture" itself implies a pattern. Olmsted stated that everything on the left of the board (linguistic) must by definition be a subclass of something on the right (culture). Hockett did not think switching terms would work because anthropologists have not arrived at comparable concepts. Personality is the closest correspondence, but "personal culture," the individual's total repertoire of culture, would be preferable. Lévi-Strauss suggested as correspondences to a limited extent: for idiolect, "tribal pattern"; for common core, "cultural pattern" (attempt to configurate several tribal common cores); for overall pattern, "areal pattern" (attempt to configurate the overall patterns of several cultural areas). Hockett commented that since the linguistic terms do not refer to operations between languages, cultural comparisons which cross boundaries are not analagous to what was originally intended. Bar-Hillel asked why there need be any analogy between linguistic concepts and cultural concepts. Hockett agreed that perhaps the linguistic system should not be made analagous to culture. Hill said that the work of linguists at the Foreign Service Institute has shown that for one part of learned behavior close to language, analogy works well. Let's see if it can be pushed further; then one can begin to talk about meaning. Hjelmslev said that common core must have some connection with Saussure's concept of *langue*, and idiolect must be at least part

²² David Bidney, Indiana University.

of *parole*. Saussure's distinction as it stands is ambiguous; H. Paul's distinction between norm and usage might be more useful. There must be some connection between these new notions and the older European terminology. Perhaps one may speak of norm as the system of the given language; usage, or idiolect, as a possible utilization of the norm; and a calculus of the general possibilities of usage of the norm. From a linguist's point of view, the right hand group on the board corresponds to meaning. It is quite natural that, within the field of meaning, there would be structures corresponding to the structures of linguistic form. Voegelin proposed equating usage to overt culture, norm to culture, and calculus to ethos. Osgood offered parallel definitions of a language and culture trait.

In the following chart of equated terms, I = Twaddell's and II = Hjelmslev's terms for linguistics; III = Voegelin's, IV = Lévi-Strauss', and V = Bidney's terms for culture.

| I | II | III | IV | V |
|--------------------|----------|---|------------------|---|
| 1) IDIOLECT | USAGE | PERSONALITY (= personal culture) and/or OVERT CULTURE | TRIBAL PATTERN | [without parallel in culture?] |
| 2) COMMON CORE | NORM | COVERT CULTURE | CULTURAL PATTERN | REAL CULTURE |
| 3) OVERALL PATTERN | CALCULUS | ETHOS | AREAL PATTERN | CULTURE CONSTRUCT |
| 4) | | | | IDEAL CULTURE [without parallel in linguistics] |

4. Bilingualism and Acculturation

4.1.1. Introduction (Haas). Haas said that this topic should be conceived within the broader framework of two general problems: how languages influence each other; and how cultures influence each other. A typical definition of a bilingual is Barker's: a person with native-like control of two languages, coupled with the use of both in daily life. Most studies of acculturation are concerned only with the impact of European culture on non-European. While all right, these approaches are far from including all the content one wants to include. One wants a time as well as space dimension, and to cover all kinds of cultural influence. Kinds of bilinguals can be defined in terms of three dichotomies:

(R) receiving : sending (S)
(O) oral : visual (V)
(C) close : distant (D)

This leads to various types. A receiving oral bilingual (RO) can hear but not speak a second language. A receiving visual bilingual (RV) can read, but not speak. A receiving sending oral bilingual (RSO) can understand and speak but not read. A receiving sending visual bilingual (RSV) can read and write but not speak. A receiving sending oral visual bilingual (RSOV) can speak, understand, read and write. A close receiving oral bilingual (CRO) meets the language in di-

rect contact. A distant receiving oral bilingual (DRO) would be illustrated by a person who understood foreign radio programs. A distant receiving visual bilingual (DRV) in space might read foreign newspapers, and in time might know Latin or Greek. Further types could be set up. Since all influence of one culture on another is through some communication medium, we need to consider all such cases of bilingualism, or some other all-inclusive term. Even a schoolboy's knowledge of Latin is a kind of bilingualism. The trouble in the past has been that we were not willing to consider all kinds of contact. Bilingualism may involve non-parallel relationships. A person who speaks language X and only understands language Y can communicate bilingually only with a person who understands language X. One may have a person who sends and receives in French, and only receives in German, and only receives in French. In a bilingual community one often gets an "inside" language X and an outside language Y, frequently in contact with a monolingual village which speaks only Y.

4.1.2. Discussion. Discussion revolved around a number of problems, including cultural attitudes toward bilingualism, inner speech, bilingual communities, second-language learning by children, and linguistic aspects of acculturation.

French mentioned factors affecting bilingualism among Indian groups. Personal confidence may determine whether an individual will use his limited ability in another language or not. Stereotypes may exist that one language is easy to learn or difficult to learn. "Easy" is a rationalization based on cultural factors, including many communications with speakers of this language; population size, trade, political domination are some of the cultural factors that make or don't make for bilingualism. Kiparsky mentioned the prestige given bilingualism in some European countries, so that a bilingual will be accused of having only one language really, and become angry. Jakobson raised the problem of which of two languages is used by a bilingual in inner speech. If one language is from the mother and one from the father, the two may retain different functions. There are many cases where it is impossible for an individual to say which is his basic language. Haas reported on a prince of the Siamese royal family, who, when asked what language he thought in, said, "I can't think." French discussed this as illustrating a real problem; when acculturation progresses rapidly, situations arise where parents can't discuss important subjects with children, since they lack the terminology. Some bilinguals have said that they lacked the vocabulary in either language to do certain kinds of conceptualizing. Kiparsky asked how it was possible that a person could speak a language but not understand it, as himself with English. Haas classified the case as of a sending oral bilingual, but thought it seemed incredible. Sommerfelt raised the question of the language in which a person dreams. Jakobson cited cases of completely bilingual tribes, where bilingualism was a basic tribal activity. He also cited certain trilingual communities in Eastern Latvia, and Jewish groups who spoke Yiddish, Hungarian, and Slovak. Voegelin mentioned cases of non-reciprocal intelligibility, as when the Achumawi understand the Atsugewi but not the other way around.

Smith discussed the problem of second-language learning by children who live in foreign countries: a child under twelve will try to lose the other language im-

mediately, as soon as he gets back to this country; it disappears in six months. From 12 to 18, girls who pick up the other language will keep it, being proud of it and because it helps in school. Boys will lose it if they do not practice and will keep it quiet. For the child it is a question of identity: "who am I?" Parents ask if learning the second language will hurt the child psychologically. According to Jakobson, there is no trouble if the child associates a language with one group of people; if he can keep the two languages apart, there is no trouble about identity. He remarked that if both parents speak one language to a child, there is no problem, but if the same person speaks two different languages to a child, there may be even pathological results.

Osgood discussed the use of language measurements as indices of degrees of acculturation. There are eight stages, which may parallel the steps in childhood learning. At the lowest rung, (1) a person can decode lexical information in context (but not get structure); at (2) decode lexical and structural information (including grammatical tags, order, etc.; context becomes less important); (3) encode simple semantic units in context; (4) encode semantic and structural units; (5) internalize a new decoding and encoding system (thinking in the language); (6) discriminate fine structural distinctions; (7) discriminate fine semantic distinctions; (8) discriminate subtle situational contexts for lexical items. Actual measures for these would include, some comprehension test of sequential messages; test labelling, vocabulary, efficiency in making wants known; standard fluency measures; use of free association tests—there tends to be a stable latency within one language; choice sets; standard similarity tests; a test of semantic differentials devised by Osgood.

Barker presented a paper on correlations between social behavior and linguistic usage, comprising the results of research among the Mexican-American population of Tucson.²³ Olmsted reported his attempt at such a study among the Navaho, dividing speakers into five classes according to the degree of English control.

4.2.1. Introduction (Casagrande). Casagrande took up the relation of language, not to the individual, but to the larger social group, and also the effects on language itself of bilingualism and culture contact. He posed two questions: (1) what happens to language when two groups bearing distinct cultures come together; and (2) what of use to cultural anthropologists in their study of acculturation and culture contact can be gotten from study of the languages involved. More could be predicted about what will happen to the languages by knowing the conditions of contact than from a knowledge of the languages' structures. Results vary with the conditions of contact, according to its extent, the relative dominance and size of the groups, the types of persons in contact, and so forth. Regarding vocabulary, Comanche shows four ways of extending it to meet new products of another culture, and these same devices are used by other groups: (1) extending the meaning of native words, by analogy of appearance, function, and so forth; (2) new coinages, either descriptive or non-descriptive; (3) loan

²³ George C. Barker, *Bilingualism and the Ethnic Group: A Linguistic Approach to Social Differentiation*.

words; (4) loan-translations. Deductions about the culture can be made from the way in which the resulting new terms are used, as when a single Comanche term for doctor and minister relates the native union of these two functions in a single individual. Different kinds of acquisition from different cultures are reflected in the lexical borrowing. Some of the loan-words become productive, sometimes replacing older words; also, words by different devices may co-exist, as when Comanche simultaneously had three words for spoon, each the result of a different process. In Comanche, the names for northerly animals contain short, analyzable stems, while names for southerly animals contain many descriptive, analyzable stems. This indicates the northern provenience of the Comanche, since the unanalyzable stems would be the older.

4.2.2. Discussion. In the discussion, other factors in linguistic acculturation were pointed out; the question of prediction was raised; the inferring of Comanche northern provenience and other problems of method were commented on; and the relative roles of language and culture were considered at length.

Haas showed by a Hupa example that the effect of native linguistic tabus must be taken into account when inferences are made about the origin of new terms. These tabus may also produce new coinages, as when a plant or animal must be renamed if a person bearing the name dies. Shimkin cautioned against deriving more than an unknown fraction of Comanche loan-translations from direct Comanche cultural experience; there was a great influence of Indian groups upon each other, especially through sign language. Lotz described an even more complicated situation in the establishment of literacy and standard languages in Europe in the eighteenth and nineteenth centuries. He suggested a classification which he felt more in accord with the demand of linguistics to separate the purely signal factors from the functional. French gave illustrations from Northwest Coast tribes of complex situations; one Indian language may be first dominated by another Indian language, then by the language of the whites; if two Indian groups speak a common third language, as Chinook Jargon, the original languages may drop out with time, leaving the white language and the common language; and people will say that the common language was the language of their ancestors.

Sommerfelt brought out the role of prestige in linguistic acculturation. Lounsbury stressed the two factors of prestige and prejudice. Regarding Smith's description of the reaction of American children to bilingualism, children from other cultures need not react the same way. Regarding the sociology of language replacement and historical implications, the substratum theory, discredited because of misuse, can still be applied. When a small minority, whose language has great prestige, dominates a large majority, there may be replacement of the latter's language without complete achievement of the patterns of the prestige language. In Peru, the influence of Quechua persists in the Spanish accepted by Indians. Jakobson added the desire to have a new word rather than a traditional word as another factor, besides prestige and prejudice, in innovation. Lotz proposed three factors determining the outcome of linguistic acculturation in Hungarian: nationalism, aesthetic approach, and competing common forms, although it is difficult to say that the linguistic factor is lowest in rank. Sommerfelt cited

a Norwegian instance in which there were both linguistic and nationalist factors. Bar-Hillel asked if it could be predicted which of two competing terms would win under given conditions? Hill said that one law known was that of "dangerous homonymy". Weltfish stated that, until one could so predict, there was not a science of linguistics. Lotz cited a case in Hungarian of competing forms. Wells said that the Comanche evidence as given could be interpreted in the opposite way, explaining the complex descriptive names as those for familiar northern animals, and names for southern animals as loan-words, and for that reason unanalyzable. Further evidence, of course, would decide between the two hypotheses. Casagrande replied that the descriptive pattern was that normally used in Comanche for new items and that there were only a few examples of loan-words. Shimkin brought out some of the considerable direct historical evidence for the northern provenience of the Comanche. Olmsted commented that a way to check Wells' hypothesis was to determine whether or not the descriptive names existed in other Shoshonean languages; if so, they were the older. Wells presented four general ways of determining the presence of loan-words: (1) variation within the language; (2) comparison of different languages—if the word occurs in a cognate language, it is not borrowed, and if it occurs in a non-cognate language, it is a loan-word; (3) phonological structure; (4) if it is one of the most frequent words in the language, generally it is not borrowed, though this hypothesis needs further study. Lotz cited Hungarian, where the most common verb, for "to do", was replaced by a Slavic loan word. Weinreich asked if Casagrande thought a difference in cultural attitude was reflected by the frequency with which names were derived by one device or another. Often a language has standards for the mechanisms to be used in naming new objects, regardless of the cultural situation. Casagrande and Lounsbury agreed that inference about cultural attitudes could be made, if done with caution. Bar-Hillel asked if it made sense to say that certain languages are more susceptible to borrowing than others. Sommerfelt cited Icelandic, which translates all foreign words; there is a conscious attitude opposing borrowing, and also a structure into which it is difficult to integrate foreign words. Hjelmslev referred to Albanian, which has an immense number of loan-words and only a relatively small native stock. It does not make sense to say that the language is borrowing words, but rather the community, since all Albanians are bilingual or trilingual. In Finnish, the opposition to loan-words might be attributed to structure, but in former times the same structure was receptive to Germanic loans. This is evidence that the reasons for opposing loan-words are anthropological rather than linguistic. Lévi-Strauss presented several instances of innovations for which the explanation would be in one case purely anthropological, in others either primarily linguistic, or jointly anthropological and linguistic. Voegelin quoted Ralph Beals as saying that acculturational linguistic work had given information but was low in theoretical interest. This recalled that much of the work had been done by persons who were primarily culturalists. It might be related to linguistic work with a theoretical bearing if the kind of borrowing being considered were thought of not as something done before or after the usual cultural or linguistic description, but as something integrated into the description

of the structure of the language. One could consider open morpheme classes, which were open to borrowing, and closed morpheme classes, which were not. Osgood raised some of the general questions which must be considered if the field is to be explored fully: (1) Under what language and cultural situations are innovations made? (2) In what ways are innovations accomplished? (3) How does the existing nature of the indigenous language condition innovation? (4) What kinds of content (meaning) are most easily borrowed? (5) What changes in meaning are likely to occur under various conditions?

5. Linguistics and Psychology

5.1.1. Introduction (Lounsbury). Lounsbury presented certain ideas drawn from perception psychology which have recently been influencing anthropological thought about language through their use to support and extend ideas derived from Whorf, Lee, and, ultimately, Sapir. These ideas concern a world view or implicit metaphysics supposed to be contained in language, which causes people to perceive and respond to situations in ways which influence other cultural behavior. First, perception is at least partly learned; it is not equivalent to sensory reception, which can occur without perception taking place. Second, perception can only be inferred from overt behavior, from differential responses to different stimuli. Not every stimulus gives rise to perception and not every perceived stimulus gives rise to overt responses; the experimenter, then, cannot know all the facts of perception. With himself as subject, he can observe otherwise inaccessible data, but there are difficulties in setting up controls. Third, although Hallowell and others have discussed the influence of language on perception, another term, dealing directly with the responses rather than with something which must be inferred from them, is preferable. Since we can know whether a subject attends to, takes account of, a situation, Lounsbury would substitute "attention". Fourth, an organism does not respond to all potential stimuli, but there is a screening which is at least partly learned. A useful concept is that of the defined situation. For an individual, a defined situation is one which contains some features to which he has learned to respond in an at least partially consistent or predictable manner. These may be called cues in the situation, or the distinctive features. Others, to which he has not learned to respond, may be called background features, or non-distinctive features. An undefined situation is one which contains no such features; response is either absent or random. A culturally defined situation is one to which members of a society have learned to respond with some degree of consistency and social conformity, as opposed to what we might call an idiosyncratically-defined situation. Fifth, screening results in classification of equivalent classes, consisting of equivalent or non-contrasting situations. If two objectively different stimuli, S_1 and S_2 , elicit the same response by virtue of containing the same cues and differing only by features which are not cues, then they constitute a class of equivalent stimuli so far as response is concerned. Sixth, defined situation and classification are found at both ends of language behavior. A stimulus gives rise to a linguistic response, which in turn is stimulus to another response. A good example is the way in which different

societies classify kinship. The anthropologist usually defines kinship classes by naming the members of the class, but another way is to give the defining features of the class, those conditions which all members of the class fulfill. In these defining features we have things which are responded to differentially for purposes of linguistic responses. Seventh, there are at least two levels of semantic classification through language. The primary level is direct classification of environment and experience through lexicon. The secondary level is classification of items and propositions in terms of logically arbitrary but obligatory categories in language of the sort Sapir called concrete relational concepts. Eighth, what are the implications of these types of semantic classification? Some have assumed that these obligatory classifications mold the thought and logic, or illogic, as well as other aspects of the behavior of people. Some make a slightly different assumption of a deep psychological connection between all patterns of behavior, including those of language, as if cast from the same mold. Some such assumptions seem implicit in the hypothesis of Lévi-Strauss concerning the correlation between the mode of approaching problems of social organization and that of approaching problems of communication. Hoijer, more conservatively, seems to prefer to discuss how culture may affect language, rather than the reverse. We can ask, then, do semantic classifications affect perception and other behavioral responses, or do other types of behavior steer the types of innovations made in language, or does it work both ways? All three are plausible, but Lounsbury takes a dim view of everything written on this subject which has gotten widespread popular attention. It is not valid to analyze the semantic categories of a language and then say we have the world-view of the culture, as Lee does for Wintu. We should have to formulate a hypothesis of such a connection which can be tested. This would require finding in some non-linguistic part of culture correlates to the linguistic features. It is not enough to find similarity between some one aspect of culture and some one semantic aspect of the language, as all articles so far have done. We need cross-linguistic comparisons and statistical studies to establish a correlation. Also, we should recognize the possibility of linguistic lag. There need not be a correlation between all linguistic patterns and the contemporary culture. When a classification in the language does not now have a correlate in the culture, however, one might assume that there had been such a correlate once. The concept of linguistic lag can be illustrated by several examples of gender in European languages. Although it has become almost pure relational, in Sapir's term, there are instances in Portuguese where it still has concrete relational function and where with native speakers it still gives some basis for analogizing. One should look for correlates of semantic categories in what people are attending to when responding linguistically. The same holds for different kinship terminologies; it is only for certain linguistic responses that no differentiation is made. One should be cautious about neutralization and specification of semantic oppositions, according to what people attend to in particular situations. Two lines of investigation might be fruitful. Because of linguistic lag, the investigation of categories seems less productive than investigation of the primary classification of experience, environment, etc., that occurs in lexicon. The neglect of lexicography is

deplorable; if rejection of the study of meaning was once helpful for the development of linguistics, we can now reconsider the possibility of a scientific semantics. The context analysis of texts may also be valuable.

5.1.2. Discussion. Discussion took up the question of grammatical categories, particularly gender; color-perception and linguistic classification; and the stimulus-response diagram.

Jakobson said Boas had formulated the question when he distinguished the meaning of grammatical categories as something compulsory, from lexical meaning, which was optional. He stressed the great psychological importance of the choices which a speaker must make because of the compulsory grammatical categories of his language. Gender, for example, plays an enormous role in Russian. All grammatical categories exert a strong linguistic pressure on our observations; but this does not mean different languages have different *Weltanschauungs* and cannot understand each other. Any linguistic category is a substitute for a more complicated expression. In scientific use, we can set up logical definitions of terms and so forth, but in other uses grammatical categories play a tremendous role. Osgood showed the different implication of pairing "good" and "nice" alternately with "man" and "woman." Here English, without a gender category, achieves a similar end by connotation, association. How does this kind of non-grammatical, non-required effect work with the deliberately coded grammatical implications? Two reasons why some kinds of meaning become deliberately coded into messages and others not might be: (1) that those which had to be repeatedly used became codified, as in a situation where gender had to be repeatedly specified because of its importance for behavior; that is, such frequently coded lexical meanings became non-lexical tags; (2) where a particular kind of expectancy has to be maintained over a long period of time, as the tense of an utterance. Sommerfelt pointed out that there are categories which make certain classifications possible in some languages and not in others. Regarding gender, it is not completely dead in European languages, as a Norwegian example shows. Lotz explained the difference in approach to gender of Jakobson and Hjelmslev, in terms of the different dichotomies involved in the gender of German and Danish. Ray said that Jakobson's and Lounsbury's remarks agreed with his own research into color concepts in native North America, especially the so-called green-blue confusion. Color distinctions that can be made in any culture are vastly greater than the linguistic system permits to be expressed. People may have a pair of terms for intenser blue and intenser green, plus a single term for intermediate, which misleads observers. One finds very little correlation between the linguistic designation of color and anything else in the culture that has to do with color. There is little or no correspondence between the linguistic designation of color and color preferences. This implies a three-fold situation, comprising linguistic terminology on the one hand, gross color behavior in the culture on the other, and in between a folk science, developing a classification. This classification does affect the manner in which people think about and react to color when conscious analysis is not present, but it is not a clue to the gross cultural system. Osgood said that Lounsbury's diagram must be modified

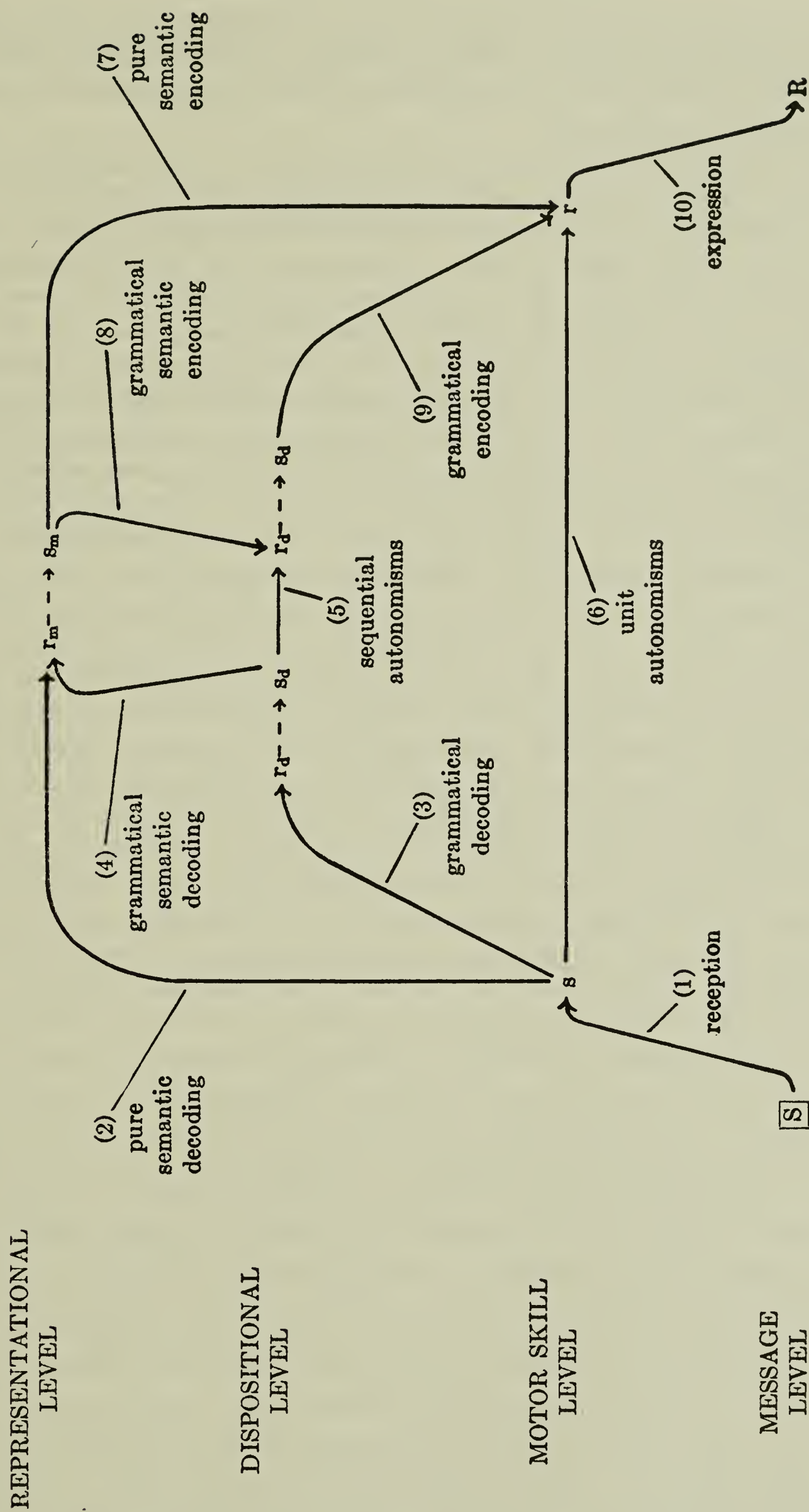
to allow for a mediational-symbolic process; what provides classification is a common symbolic process. Lounsbury replied that this cannot be observed. Olmsted suggested a historical problem framed in terms of stimulus-response diagram, where the learning of two different stimulus-response situations comes into conflict. In Russian, items which may be of either gender but are very alike in formal constituency show patterns which in other places correlate with only one of their genders; on the semantic side they are one thing, on the formal side, another. Osgood said interference here was not possible since one process was encoding, and the other process decoding.

5.2.1. Introduction (Osgood). Osgood described material contained in the second half of the paper he had prepared for the Conference, concerning the study of aphasia as a test for a theoretical model of the psychological processes involved in linguistic behavior. This work is conceived as part of psycholinguistics. If microlinguistics is the descriptive study of the structure of messages, and other social sciences study the senders and receivers of messages, psycholinguistics is the discipline which studies relations between the intentions of senders and the structural characteristics of messages and the relations between the latter and the effects produced on receivers. It is concerned with encoding and decoding processes. The paper, then, attempts to give the outlines of a comprehensive psycholinguistic theory, using aphasia studies as a test. The model is based on a mediational type of learning theory.

Summarizing the model, Osgood presented an analysis of cases of aphasia described by Head, Goldstein,²⁴ and others. From thirty-nine detailed cases, he could select twenty-eight kinds of language performances that were frequently reported, such as sentence comprehension, writing from dictation, and speech skills. A chart was constructed showing normal or disturbed performance for each patient for each of the twenty-eight performances. A contingency analysis was then made in order to test the significance of the difference between chance co-occurrence in the same patients of each kind of disturbance with any other kind of disturbance. The theoretical model makes it possible to specify what pathways are necessary for executing each performance and to predict the theoretical effects of different kinds of lesions, e.g., lesions at different points of the model. Where pathways overlap, a lesion affecting one must affect the other. Different lesions affect or eliminate different groups of performances. The types of aphasia can be classified according to such a pathway analysis in the model.

Using twenty-one possible lesions and studying each pair of language performances separately one can count the number of these twenty-one lesions which would affect the common pathways involved. On the basis of the model, one could then predict which performances should be positively related, which negatively related, and which should be independent of each other, in terms of co-occurrences in aphasic cases. When these predictions were compared with the empirical data, comprising a total of three hundred forty-nine tests (each performance with every other performance), the results gave a high confirmation;

²⁴ H. Head, *Aphasia and Kindred Disorders of Speech* (1926); Kurt Goldstein, *Language and Language Disturbances* (1948).



only nineteen out of three hundred forty-nine predictions made were negative. Of the statistically significant relations predicted, the data showed only one reversal out of seventy-four. An independent check, by investigators unfamiliar with the theory, is necessary.

5.2.2. Discussion. Discussion was directed largely at Osgood, asking questions about his work and suggesting additional aspects of such an approach.

Chao asked if the patients were English-speaking, which Osgood said they were mostly. Chao suggested that in languages with different grammatical structures the model might have to be altered. Osgood asked if data were known on aphasia in many languages. Shimkin asked if the theory was applied to functional disturbances. Osgood replied that if one could get repetitive confirmation, one could pinpoint them. Lane asked if the cases used described in what area of the brain the lesion was. Osgood replied that this did not make much difference for the theory. Bar-Hillel raised the question of partial lesion and partial inability for performance. Osgood replied that was the reason for taking only severe cases. Casagrande asked if the system then did not allow for partial lesion, which Osgood confirmed. Lotz asked if there were any neurological evidence for the model. Osgood replied that there was none for the dispositional end, which was hypothesis, although evidence could be looked for. Crucial experiments show that a two-stage process at least is essential. Jakobson said that the most important feature for the linguistic study of aphasia was stratification of grammatical categories, the order in which they would be lost. Two types of prediction can be made, that within one language, and that which applies universally. One can predict to eighty percent the order of the reacquisition of the phonemic pattern. Shimkin said that in cases of senility there seemed to be a basic process rather than the disturbing of any particular area. The results seemed to be the inhibiting of new acquisitions and a great weakening of the feedback system of recent acquisitions. Perhaps the theoretical model used by Osgood needed a third dimension, comprising circuits built up in time. Bar-Hillel also suggested a time dimension. Osgood said a time dimension would not be relevant. Weinreich said that the model applied essentially to unilingual speakers and should be extended to account for learning and aphasia in bilinguals and for switching code. Three types of disturbances and functions are involved: (1) normal functioning within one language; among bilinguals, one language may be more affected than another in loss and relearning; (2) ability to translate; (3) ability to make a clean break, switch codes without interference from the other. Osgood said that a second language could be handled in terms of hierarchies, like layers of cable. He then discussed the relearning process in victims of aphasia. It may take several months to be able to name the first object, a week or more to name four or five, and within two weeks the subject is getting them all the time. This may possibly be explained in terms of the bilateral aspects of the nervous system. Lesions knock out certain functions in the left hemisphere (for right-handed persons), but we may assume that exactly the same functions are present and organized in the other hemisphere, though silent. The process then is one of rerouting through the other hemisphere. Lotz queried whether the description of learning

language was a historical process with as little to do with language as other kinds of historicism. One of the great achievements of linguistics had been to free itself from historicism, describing language in terms of its internal dependencies. Osgood said this was still an issue in psychology, but he thought it a pseudo-problem. If one takes the gestalt conception of vectors pushing in various ways at a given point in time, one can't predict entirely from the synchronic picture. One has to know the strength of the vectors, i.e., by knowing the past behavior of the organism. Both kinds of information are necessary for a complete science.

6.1. Ethnography without Linguistics

6.1.1. Introduction (Ray). Ray said that the topic really meant ethnography *with* linguistics, and the awful consequences of ethnography without linguistics would be pointed out. One purpose of the Conference was to bring ethnography and linguistics closer together, which would be useful both on the practical and theoretical planes. Not long ago, the subject would have been inappropriate, because, both in terms of theoretical attack and practical means, some control of linguistics by ethnographers was assumed, and the doctorate not given without it. If one does not need the intensive control Sapir hoped for from his students, today many have virtually no control. The reasons are: (1) increasing specialization within anthropology and broadening of the field; and (2) greater emphasis on microlinguistics by linguists. This internal development of linguistics, granting the advance in scientific precision, has made it more difficult and less relevant to ethnography. The division of the two fields is at least partly due to the development of an esoteric language, gobbledegook, within linguistics. The attention paid to metalinguistics at the Conference, however, gratifies an anthropologist; the potentials of earlier times, when anthropology and linguistics were closer, are now greater and nearer to realization.

Two criticisms can be made of the Conference discussions: emphasis on definition for the sake of definition, and the use of analogy. Drawing definitions does not require the kind of attack involved in building a theory; they have no value for their own sake. One can only define tool-terms, and this shouldn't be confused with answering the questions that face us. A concept can only come to be known after all its complex inter-relationships have been worked out. Linguistics is of practical assistance to ethnography in obtaining data, particularly in field work. A few suggestions and examples, mostly familiar ones, can be given: (1) Elucidation of esoteric features of culture, the making clear of conceptual schemes. Ray said this was to him the most important service of linguistic tools. It did not imply getting material in text form, but using linguistic tools in whatever way applicable. (2) Kinship structures, and naming systems. (3) Studies of perception, as Ray's own work on color-perception. (4) Ethnobotany, ethnozoology, and folk-sciences generally; the materials in existing monographs are often inadequate for linguistic reasons. (5) Literature, folklore, proverbs. One need not take all material in texts to take advantage of the rapport established through the use of linguistic materials. (6) Cultural communication, a much neglected field in ethnography, because the linguistic tools for getting at the patterns involved

have not been made use of. (7) Bilingualism. (8) Special languages, as of men and women, etc. (9) Dialect relationships; the important factor is the communicative difficulty or ease with neighboring groups. (10) Culture-historical depth.

6.1.2. Discussion. The discussion centered on (1) work it would be desirable to have done, and (2) the problems of personnel, funds, training, and other obstacles, past and present.

6.1.2.1. Culture history. Shimkin developed further some of the uses of linguistics and ethnography indicated by Ray. In the field of functional semantics, the anthropologist can do much for the linguist in the patterning of value-systems. The relations of comparative philology and archaeology have been neglected in this country. As an example of their coordination he cited work in Central Russia where the sequence of loan-words fits in well with the archaeological sequence. Archaeological evidence shows that certain loan-words must have displaced native terms, since the materials named were already present; the linguistic borrowing does not indicate the original cultural borrowing. Philology gives evidence for the direction of the loan. Many North American problems, such as the Sun Dance, have not been attacked seriously with linguistic data. This is due to a failing in linguistics, which has neglected getting enough lexical data; there are not enough large, consistent dictionaries with the data needed for sound archaeological work. Haas denied the neglect. Many linguists had gone into descriptive work because of their deep interest in doing something about these problems; the material must be gotten first. Some work has been done in North America on Athapaskan and Algonquian. The results seem slight because a good complete description of every language in North America is needed, and there are only a dozen or two dozen workers. A complete job may take many years of work. It isn't that linguists don't want to do all these things, but how? Weltfish suggested that a sufficient public climate of opinion in support of the problems, in order to get money and thus scientific workers, had not been created. Regarding culture history, one should think about the popularization, not the vulgarization, of science. A good deal about the conceptual problems of language is found in the problems which relate linguistics and archaeology, for example, archaeological evidence of polylingualism; these are not humble at all. Definite research purposes should be formulated and pushed. Haas agreed with Weltfish's remarks about polylingualism; these and many other kinds of studies should be made. An additional handicap is that most linguists are still dependent on the kind of jobs they took during the war; people urge them to spend all their time on such work, arguing that it is much more important. Regarding popularization, there was success in the teaching of practical linguistics; but how can culture history and the North American Indian be made a practical problem?

Kiparsky commented on Shimkin's example of the work of Finnish scholars regarding Russian cultural history. The first work was done by the Danish scholar Thomsen; also, it now seems that the picture given by Thomsen and the Finnish scholars is not as clear as imagined, and the archaeological details cannot be coordinated with the linguistic data. Garvin²⁵ said it was all very well to cor-

²⁵ Paul L. Garvin, Georgetown University.

relate linguistics and archaeology in the Old World, where a few language families were spread over a large area. In North America there are gallons of language families, each with one or two gallons of languages, all difficult to work with, spoken by small groups in limited areas. This linguistic pattern makes it much more difficult to do culture-history in North America. Also, research in North America is not supported by nationalism and government interest. One can't make a case for American nationalism by pointing out the deep roots of the Tunica in the Southeast. Shimkin considered the comments about applying linguistics to cultural anthropology unduly pessimistic, involving an attitude of perfectionism. The view that one need know every language to do culture history does not hold water. Requirements can be trimmed to a useful level. Linguists' data are now often of little use to cultural anthropologists because of lack of precision in obtaining lexical meaning for forms. Mutual cooperation, not mutual recrimination, is needed. A complete cultural history of any part of the world is an unreachable goal, but despite the diversity of languages in North America, there are a limited number of spots (Uto-Aztecan, Siouan, Algonquian) where cultural historical analysis would give a great many very important points. One should not think the problems must be postponed; in this country, there is already some interest in archaeology because of very useful practical applications. Gleason made an appeal to encourage contributions from missionaries, of whom there are hundreds working in languages throughout the world. Quite a few are competent in linguistics and in ethnology and might make contributions if welcomed.

6.1.2.2. Methods and personnel. Garvin defended linguistics, speaking of the opposition between methodology and relevance. With recent emphasis on methodology, some of the problems of relevance have been obscured, but emphasis is shifting now, and results would not be possible without the improvement of methodology that took place. Perhaps the idea of gobbledegook in linguistics is exaggerated by those who have as much in their own fields, such as the archaeologists. Ray agreed concerning archeologists and added he didn't mean there was no gobbledegook in ethnology; but there is not as much because the kind of unit and precision are not the same. Garvin distinguished two levels of linguistic techniques involved in practical cooperation of ethnographers and linguists. The first, or crude technical level, the ethnographer should learn in graduate training; the second, the ethnographer cannot be asked to learn: he should call in the linguistic specialist. Ray commented that for the fieldworker simply to record phonetically and bring material back for analysis would not satisfy field requirements, nor give the further leads for ethnographic inquiry that were possible. E. W. Voegelin pointed out that if one counted at the present time, quite a good deal of linguistic training was being offered in anthropology departments. C. F. Voegelin commented, "offered, but not insisted on". E. W. Voegelin asked if Ray had in mind training one person in both ethnography and linguistics, or one turning material over to a specialist. One descriptive linguist had said that the two cannot be combined, since each requires so much time. Garvin suggested very minimal requirements; what would Ray consider the minimum? Ray said

the ability to make phonetic recordings and phonemic analyses, but not to write a grammar. Gleason said one can't collect materials for someone else without knowing something about them; to phonemicize someone else's data is a rough job. It is doubtful whether people can be trained for adequate phonetic transcription without introducing them to phonemic theory and practice. Garvin agreed on the need for phonemic awareness; the remark on phonetic accuracy was made in reference to materials he had encountered where the linguist could not even identify the term recorded by the ethnographer. He further remarked that the things ethnologists want to know from linguists are usually very sophisticated, problems which one can barely solve for languages one speaks fluently, let alone those where control is slight. Phonemics and grammar can be done with relatively little data, but the ethnographer's questions are hard to answer even for languages where there is tremendous data. The problem of functional styles cannot be solved without a great deal of information and linguistic training; native cultures also have criteria for good and bad language, standard and sub-standard speech. Bidney then challenged Ray's comments on definitions and analogies. The dangers of analogical thinking can't be avoided without close analysis of concepts. There must be a maximum of caution in analyzing one's own terms. Instead of making a sharp distinction between concepts and tool-words, one should speak of tool concepts. Analysis of these can't be postponed until all the data are in. Ray replied that analysis of concepts was exactly what he was making a plea for.

6.2. Linguistics without meaning

6.2.1. Introduction (McQuown). McQuown first commented on the phrases "combinatorial linguistics" and "without meaning". If one asks what are the limitations of combinatorial linguistics, the answer is fourfold: methodologically, none; practically, those imposed by the adequacy of descriptive statements; by definition, if language and culture are divided, clearly on the side of language; by induction, wherever one discovers a major boundary of cultural systems to exist, or, for extra-cultural reasons, wherever it is convenient, as audible vs. visible phenomena, etc. Any drawing of a line is arbitrary, and the criteria should be stated. In regard to working "without meaning", meaning derives from the co-occurrence of cultural isolates within linguistics, or within the totality of cultural systems. If combinatorial techniques are extended by analogy to all of culture, then the phrase "linguistics without meaning" is meaningless. If the listing of co-occurrences is restricted to those which occur only within language, then one obviously works only with linguistic meaning.

The cultural activity of any cultural idiolect is the interworking of (1) biological equipment, (2) what is transmitted from others, (3) what one passes on to others, (4) Nature's force majeure. To understand the result at any one time, we need to know all the factors. To describe, we need know only the third. To perpetuate itself, culture cannot dispense with the third, communication. The job of the cultural anthropologist is to study cultural communication; nothing discoverable about culture can be found out in any other way.

A series of questions may be asked. (1) Through what media does communication take place? (a) If soul to soul, this is not accessible now; (b) through movement of the human body or lack of it, and ensuing movement in the air or lack of it, between two bodies; (c) any more or less durable effect of bodily movement on perceived objects.

(2) Which of the media is inherently capable of the greatest amount of discriminable internal differentiation? The auditory. (3) Which part of the human body is inherently capable of the greatest amount of discriminably different sound producing movements? The vocal organs. (4) What is the principal communicative activity of the vocal organs? Speech. (5) For communication to occur, what is required of this speech activity? That it consist of differential groupings of isolable entities, patterns, and pattern points. (6) What is this patterned grouping of pattern points in speech activity? Language. (7) What is that human activity with the greatest number of pattern points and pattern point combinations? Language. (8) What other human activities show such recurrent patternings? Non-linguistic vocal activity (vocal gesture), and non-vocal activity. (9) Are these other types of activity inherently capable of the same number of discriminable distinctions as speech activity? No. (10) What is their function in the act of communication? That of supplementing, refining or contradicting the import of the speech activity. (11) Of the various activities, which then is most adequate for the act of communication? Language. (12) Through what media then must most of the other patterns of culture manifest themselves? Language. (13) What are some of the other media? Bodily activity and vocal gesture including singing, dance, other movement, and extra-corporal effects, as writing, sculpture, painting. (14) Where in the medium of language can these other patterns of culture be observed? In the non-linguistically patterned recurrence of linguistic pattern points and, of course, the whole patterns through which they exist. (15) In what kinds of recurrent pattern points and whole patterns can these other patterns of culture be observed? The reply consists of an inventory of the things we fancy we know about the internal structure of language: (a) in the non-linguistic contrastive variation of phonological elements of which the phonemes are composed; (b) in the nonlinguistically determined selection of phonemic sequences in a quantity of discourse; (c) in the non-linguistic contrastive variation of the phonemic shape of morphemes of which language is composed. (16) Can we observe these other patterns of culture in any other way than through the use of language? Yes, we can intuit them directly; or we can practice operations similar to those of combinatorial linguistics on a mass of linguistic and nonlinguistic material without regard for the internal segmentation of each. We can determine phonemes without regard for the phonetic properties of the media; we can determine morphemes without regard for the phonemes of which they are composed; we can determine arrangements in a language without regard for the particular form-classes or sequences of which they are composed. We can discover these larger patterns of culture without any overt awareness of the units and groupings of units which constitute the medium through which they manifest themselves. On two counts, it is desirable to be sure of foundations. The larger

patterns of culture effected in these more organically-bound patterns are of the same kind, if not the same order, as these. These larger patterns of culture may be said to mean those which are discovered internally in media such as language just as justifiably as the linguistic patterns may be said to mean the larger patterns of culture. If we hope to show explicitly how the larger patterns of culture are arrived at, make our intuitions to be realities, we should ground them in what we know about the first and more basic level.

6.2.2. Discussion. The discussion centered on the nature of meaning, how meaning enters into linguistic analysis, and the levels of linguistic analysis.

Osgood asked what cultural anthropologists mean by meaning? There are at least five possible kinds of statements about meaning: (1) the lowest level, a pointing procedure or derivation from it; (2) an equation of signs (as in dictionaries); (3) meaning inferred or measured from context; (these being external); (4) formal sampling of associations an individual makes; (5) measurement of representational processes (a hypothetical procedure now); (these being internal). Would linguists say what operations they perform when they talk about meaning? Hill replied that one can differentiate three kinds of meaning for linguistics: differential meaning; translation meaning—statement of various terms or other sequences which in a large bundle of contexts can replace another; and structural meaning—what is arrived at when one shows a structural pattern outside the linguistic system. When a linguistic system is in one-to-one relation to a non-linguistic system, we have stated the meaning of each. Hockett said that the meaning of a linguistic form is the set of conventions by virtue of which it occurs with the concomitants with which it does occur. Generalizing, one can speak in this way of the meaning of any recurrable cultural act. In language there is a subdivision perhaps not present elsewhere. One can distinguish the antecedents, concomitants, and consequences of particular terms which are themselves linguistic forms, and those things which are not linguistic forms. It is the non-linguistic entities, their antecedents, concomitants, and consequences that one is more apt to use in arriving at a picture of meaning. Garvin remarked that different definitions of meaning probably will emerge, depending on what psychological system one leans to. One possible operation is to try to get meaning by free association; the results are not totally disorganized, but have something to them. Gestalt psychology distinguishes between the unit and the context; the unit has meaning, to which is added a delimitation by the context. One cannot define meaning by context alone. Osgood said he felt that linguists state meaning as they have to in terms of their methodology, but he was not satisfied with it at all. One can see where the distributive analysis of context can give evidence of a difference in meaning, but how can it give any information of the nature of the difference? Would analysis of context in an idiolect tell you how the homonyms violet (the flower) and violet (the color) differ? Smith said that on the various levels of linguistics there are ways of distinguishing meaning. Given two different sets of distribution, there is little difficulty in assigning the forms to different form classes, which gives something to work with outside microlinguistics. One would find indications in microlinguistic phonology which, when added in context

beyond the microlinguistic level, would give all sorts of leads. These things have no encapsulated meaning, but must be seen when added to other things, considering a totality in a hierarchy. Hill added that even if the hierarchy is complete, one still does not know how things are different. One has to have a good deal of structural information about cultural facts which are non-linguistic facts. Hockett said that if his definition of external meaning was accepted—the conventions which govern the non-linguistic contests in which a linguistic form occurs—then a dictionary tells what new words mean in terms of external meaning. The things listed in a dictionary after the head word are not the meaning. With phonemicized material, one could get results about grammatical structure without meaning, but they would be worth little. Even if one can analyze the internal economy of a language without meaning, the results tell nothing about meaning. One cannot get at the whole culture from the analysis of language, which is a base only, or a frame. Supposing one has to use meaning to analyze language, doesn't that still leave us free to abstract from the devices used to separate things out? Lotz restated a question of Osgood's asking linguists to produce a semantic structure, linguistically defined, which was not a simple reflection of morphemic structure. He offered such an analysis of a set of Swedish nouns. When analyzed semantically, one can construct a cube, each element of which is defined in terms of semantic components of the nouns, but the structure of the morphemes is by no means identical with this graphic representation. Lotz urged that the linguists not feel so pessimistic about semantic results; one can get structural semantic data, and it ought to be done, working from inside linguistics. He would be inclined to include semantic features within language itself. It was a necessary but fragmentary stage when linguistics was limited to the signal factor. Myers employed "semanteme" for a term, quite different from a set of individual terms, but by means of which they all be defined. "Stool", "chair", "bench", "saddle" would all be definable in terms of "seat". Ogden and Richards had the conception that if one could isolate the semantemes in a language and look at the rest of the language in terms of these, a pattern or structure would result. The rest of the words could be stripped of emotional overtones. This approach, including some procedures of Basic English, was absurd when carried to extremes, but nevertheless a real contribution. Does this sort of thing, delimitation of semantemes, fall within the linguists' scope of reference, or is it a completely different compartment of study? Hill responded that Osgood, Lotz, and Myers had all dealt with "translation meaning". Osgood, measuring the connotative value of pairs of terms, dealt with it translated from a binary scheme to a continuum, graphing the continuum as a check on translation meaning. Lotz dealt with it in a binary sense, putting it into a spatial diagram which he used for a check on translation meaning. Myers used it in a narrow sense, his operations being part of a relatively narrow sphere in linguistics, "lexicology". Weinreich suggested distinguishing cultural activities which have the function of communication and those which do not, looking for patterns specific to those things which are involved in communication. This would be convenient especially if meaning is defined as co-occurrence. McQuown saw no need for the distinction, since, as

he used the term communication, it included all cultural human activity. Gleason classed linguists into two groups, those who consider "meaning" reprehensible and those who consider it respectable. Whatever other kinds of data than distribution the first group were willing to use, they would call it other than "meaning." This is largely a matter of terminology and reaction against a kind of past linguistics. One can't segment into morphemes without meaning; after segments are obtained, all linguists agree on leaving meaning out. Garvin said the classification had shifted. Everybody now admits there is such a thing as meaning and that it has some bearing on language, but the first group now says meaning is not part of linguistics in the narrow sense, postponing it or leaving it to others considered better qualified.

In the course of the discussion of linguistic methodology, Chao told the following story, the point of which was that a morphemic analysis of this text would require a knowledge of the orthography as well and that this reflects on the question of the independent analysis of the various levels.

施氏食獅史

石²室²詩¹士⁴施⁴氏⁴嗜⁴獅¹誓⁴食²十²獅¹氏⁴
 時²時²適⁴市⁴氏⁴十²時²適⁴市⁴時²適⁴十²碩²獅¹
 適⁴市⁴是⁴時²氏⁴視⁴是⁴十²獅¹恃⁴十²石²矢³勢⁴
 使³是⁴十²獅¹逝⁴世⁴氏⁴拾⁴屍¹適⁴石²室²石²室²
 濕¹氏⁴使³侍⁴試⁴拭⁴石²室²侍⁴拭⁴石²室²氏⁴始⁴
 試⁴食²獅¹屍¹食²時²始⁴識⁴是⁴十²碩²獅¹屍¹實⁴
 十²碩²石²獅¹屍¹是⁴時²氏⁴始⁴識⁴是⁴實⁴事⁴實⁴
 試⁴釋⁴是⁴事⁴

*碩 *shih² ~ shuo⁴*

The Story of Mr. Shih Eating Lions

"Stone House poet Mr. Shih was fond of lions and resolved to eat ten lions. The gentleman from time to time went to market to look for lions. When at ten o'clock he went to market, it happened that ten big lions went to market. Thereupon the gentleman looked at the ten lions and, relying on the momenta of ten stone arrows, caused the ten lions to die. The gentleman picked up the lions' bodies and went to the stone house. The stone house was wet and he made the servant try and wipe the stone house. When the servant wiped the stone house, the gentleman began to try to eat the lions' bodies. When he ate them, he began to realize that those ten big lions' bodies were really ten big stone lions' bodies. Now he began to understand that this was really the fact of the case. Try and explain this matter."

The text is in the so-called literary Chinese, which normally is 90 to 100% auditorily intelligible. By mere chance, constructions like English *I stay from two to two to two, too* can also occur in literary Chinese, only much more frequently.

Hence it is possible to construct a whole story with one syllable on four tones.²⁶ Then it will not be auditorily intelligible and the identification of the morphemes will have to depend on orthographical distinctions.

7. Language and Thought

7.1.1. Introduction (Bar-Hillel). Bar-Hillel presented a paper on machine translation. Various linguistic problems are encountered in the work with machine translation that have not been attacked satisfactorily before. With the machines of the present or near future, semantic considerations are completely eliminated, and they are an ideal touchstone for the application of structural linguistics. Machines work with printed materials only. Four problems arise which may be called (1) operational syntax, (2) intertranslatability of all natural languages, (3) idioms, and (4) universal syntactic categories. The first might be better called "sequentially-programmed syntax". The need is to determine the syntactic structure of any given sentence in the FL (language from which translation is made) to any degree of explicitness. The machine's operations of matching and counting, plus a few minor additions, suffice for full understanding of the grammar of any source language as presented by the structural linguist. There is an important difference between knowledge about the properties of materials, and knowledge of a set of instructions or operations. A chemist who knew all the properties of the chemical elements would not know how to proceed, what operations to perform, in order to chemically analyze a substance. The machine, supplied with a sequentially-programmed syntax as a set of instructions, can perform all the operations necessary for translation. There are no such syntactic analyses available for any language, but their preparation should be a highly rewarding task. The concept of the intertranslatability of words, as a theory, is equivalent to the theory of the universality of all natural languages. It can be understood in two mutually exclusive ways, one in which it is false, and one in which it is true, but trivially so. If a natural language is understood as a closed language, it would be impossible to translate, say, a book on quantum mechanics in the English available in 1890; it could not be done unless the translator were able to extend the existing vocabulary. In this sense the theory is false. If natural language is understood as an open language, so that the translator can add any consistent vocabulary and rules necessary, the theory is true. Under no restricted extendability does it seem plausible that any smaller unit than the sentence can be uniquely translated into any other language; the sentence might even be too short. An idiom is defined in dictionaries as unique to the language by virtue of its grammatical structure or vocabulary content, and as the whole having a meaning more than the conjoint meaning of the elements. One could conceive the machine proceeding word by word, but this collapses at idioms. In a language one does not know, an idiom would exist when none of the elements of the TL (language into which one translates) that correlate with the FL give a satisfactory translation. An expression of the FL, then, is an idiom with respect to a TL and a

²⁶ First tone high level, 2nd tone high rising, 3rd tone low dipping, 4th wide range falling, each tone being indicated by the superscripts over the characters.

set of translation rules. The only method of translating idioms by machine is not to have them. Perhaps relative to a given dictionary and set of grammatical rules, an expression may be untranslatable; one has only to change the set of rules. There are several methods, each sufficient; an optimum method might include all. For example, one may enlarge the list of correlates in the TL to include some of those in the FL. For German expressions containing *es gibt*, de-idiomized by adding to the English equivalents for *es* 'there', and for *gibt* 'is', 'are'. The only objection to such a dictionary is that it works too well. Besides the desired equivalence, many gratuitous ones are added; many possible translations arise which are incorrect. Even if inspection of context could eliminate undesirable translations, the convenience of this method is a price too high to pay, unless the equivalences occur in a large number of different contexts. Or, one may suppose a special phrase dictionary, *es gibt* having 'there is', 'there are' among its equivalences. The first instruction to the machine would be to search the phrase dictionary, then the word dictionary if needed.

7.1.2. Discussion. Discussion was directed to the points raised by the introducer.

Twaddell commented that Bar-Hillel had presented an assignment enough for any Alexander seeking new worlds to conquer: the solution of certain important tactical problems without some of the usual resources linguists and anthropologists hitherto considered essential; the solution of syntactic problems with some phonological data at least arbitrarily excluded by accidents of orthography; investigation of some kinds of messages with complete exclusion of any co-occurrence in the natural or cultural domains; exclusion of that informant who can answer the question, "Gibt es nun Wunderschift?"

French inquired what some of the aims of machine translation are. Bar-Hillel mentioned practical situations where there is not enough manpower; with machines one can use human brains that do not need to know the original language. Wells suggested that, in addition to the problems of word-by-word translation and idioms, there is the problem of ambiguity, even where there is no question of word-for-word equivalence or of idiom. Bar-Hillel replied that if the original expression is inherently ambiguous, the machine should not be required to solve it. Shimkin raised three questions: (1) in almost no case is there the possibility of unique translation—there are always a number of possible variants; (2) there is difficulty in knowing whether the unit of translation is the sentence, paragraph or longer—human beings can scan ahead, or back, for many pages to get the necessary clue to a difficult point, but for machines to scan so much would cause great practical difficulties; (3) in technical language neologisms are to be expected as the rule. It often requires a large amount of background data to make a correct selection of equivalent terms. It is questionable that a unilingual can achieve a judgment of this sort of thing. Bar-Hillel considered the points well-taken, and said he had been much concerned about each of them. It might be necessary to have the machine mark the places where neologisms occur, for them to be supplied by a reader afterwards. One could have special dictionaries for technical fields. Hahn²⁷ asked, if the machine can't do everything, what good is it? Situa-

²⁷ E. Adelaide Hahn, Hunter College.

tions which would be ambiguous to the machine would not be so to a sensible person. Written ambiguities might not be so if spoken, and the human reader can supply the spoken version. Some things are ambiguous only if one does not know the meaning of the words. In Latin, a particular sentence would have two different meanings, depending on the period. Latin meanings differ with punctuation and with quantity, which is not usually written. Regarding manpower, it takes a lot to prepare the machines; what good are they if they can't do the job? Weinreich said that the great advantage of the human translator is that he can adjust the unit of translation as the need arises. Can machines be prepared to do so? Bar-Hillel repeated the procedure of having the machine check for idioms, then scan word-by-word. He had tried to get good information on how human translator; work, but the best don't know how they do it. Weinreich asked that the distinction between an intralingual and an interlingual idiom be clarified. Chao said that "to make friends" was an idiom in Chinese. Bar-Hillel commented that if word-for-word translation of this then made sense in Chinese, it was an interlingual idiom. Weinreich said that if an idiom can only be defined with respect to the limitations of any one dictionary, then a full dictionary would exclude all idioms. Osgood suggested that idiom is an extreme example of something always present. He gave as an example different contexts in which "light" might occur, terming this the modulational effect of context. How would the machine decide? Would it have to give all the possibilities, including "light" used to indicate an object, a space of illumination, and so forth? Bar-Hillel asked if he were talking about translation or interpretation. The problem is purely a mechanical one, depending on the man who prepares the dictionary, not the man who reads the machine's results. Garvin suggested that procedures depend largely on the way in which new vocabulary is formed in a language. Gleason considered Bar-Hillel's first point the most significant one for structural linguists. They have succeeded in making statements from which one can synthesize constructions in a given language, but have not succeeded in making statements by which one can analyze constructions, according to the introducer. If so, this is a serious deficiency; but, though linguists may not be as far along in analytic as synthetic work, in certain languages, at least, one can do a great deal already. Lotz asked about the translation of Latin, which lacks definite articles, into English. Bar-Hillel said the best possibility was a general instruction to the machine to add one of the articles. Lotz pointed out that one might have to add distinctions required in the TL not specified in the FL. Bar-Hillel replied that one would translate by all possibilities; the post-reader would make the choice by context. It is shown experimentally that a greater number of possibilities does not interfere with the judgment of the post-editor, who can easily pick out the correct one. Lotz suggested the possibility of two machines translating into each other. Bar-Hillel expected a retranslation to be synonymous with the original. Hill mentioned capitalization of nouns in German as a graphic clue for the machine; it seems inconvenient that other languages do not do the same at least for machine translations. Also, it seems possible to design a machine that could make use of differential probability in judging alternatives. Bar-Hillel commented that, in principle, one can do German without capitalization, and that there exist no first class frequency counts on

which one can rely. These would seem a task of many, many years. Checking German dictionaries, he found no consistent principle in the order in which correspondences were given. The machine can, if a given translation is selected several times consecutively, from then on select only that one; that is, one can arrange a short-range memory for the machine. Ray said that many of the technical problems had been overemphasized in the discussion. The job that the machine does is not a literary job, but a practical translation, which is also of theoretical interest to linguists. Literal translation of a large number of idioms would be understandable; if so, one doesn't need to worry about a great many of the subtleties, and many of the technical problems are reduced. Bar-Hillel said he agreed perfectly. Given a text, if one takes the first lexical correlate found, the result is not unintelligible. Psycholinguists would have to investigate the extent to which people comprehended. Ray said he assumed a relatively small number of people would use the results; for a wide audience, a different attitude would be necessary. Shimkin, giving examples from Soviet languages, asked if machines might be able to handle cases in which more than one syntactical system was involved. Could they operate on more than one level of syntactical discrimination? What if a machine set to translate for Mongol rules suddenly met an occurrence of Russian rules? Bar-Hillel said a complete Russian syntax could be also on hand, so that if translation could not proceed according to the first set of rules, the machine could switch to the second.

7.2.1. Introduction (Wells). Wells spoke about Whorf's ideas that languages influence thinking. More and more people are becoming skeptical of them. By presenting a concrete example, it can be shown how these can be refined and improved. Whorf spoke of grammar as the source of the influence of language on thought. By this, he meant more than the usual sense of combinations of formal elements, but also the way in which these elements are selected to describe given experiences. In different languages there are different formal grammars and also different parts of a total situation are selected for expression. Whorf spoke of the logic of Indo-European languages and credited Aristotle and his followers with having worked out this logic. This can be tested in the history of philosophy. According to Whorf, Indo-European languages agree in general that the subject-predicate construction is part of their grammar (in the narrower sense). What influence has this linguistic fact had on thinking? It was noted by Aristotle, but did he draw philosophical consequences from it? He did and did not. He did in linking different kinds of sentences together, stressing the similarities where modern philosophers stress the differences; in giving examples, he tended to give those of the first kind:

- (1) Socrates is snub-nosed.
- (2) Socrates is the husband of Xanthippe.
- (3) Socrates taught philosophy to Plato.
- (4) Socrates is more like Plato than Cicero is like Caesar.

The modern view is that he was influenced by this kind of structure. Sayce said that had Aristotle been a Mexican, his logic would have had an entirely different form. Several modern philosophers share this idea of language influencing thought

and Whorf could have gotten it from any one of them, but perhaps most likely from Whitehead. This view is bad history. Neither Aristotle nor his followers among the medievals drew the conclusions that they might have from this linguistic fact. They never said that a situation described in the manner of (2) or (3) is essentially like situation (1), though they tended to assume it in drawing examples predominantly from (1). Nothing was made of this fact until Leibniz in the seventeenth century, who built a philosophy on the premise that every true proposition is analyzable down to a predication of an individual. It was one of the founders of modern science who drew the inference from this linguistic fact. Whorf suggests our modern science is dependent on the philosophy of those who speak Indo-European languages. This is false. The first steps of modern science were made before the linguistic theory was called into question, by the followers of William of Occam in the fourteenth and fifteenth centuries. The discoveries arose not from the critique of language but from the critiques of Aristotle's doctrine of impetus, concerning natural and forced motion. Two thousand years passed between the clear enunciation of the linguistic fact and a rigorous philosophical investigation of it; in the interval, modern physics and science were underway. One may conclude that thought of this kind in question here may vary independently of culture, and anything found about the influence of language on culture, or culture on language, may not apply to philosophical thought. We find here a fact of language affecting a fact of thought, but it is high level thought, technical philosophy. It doesn't show that the fact of language influences everyday thinking, culture, or science, as Whorf thought. Another conclusion is that we need to be able to speak of types over long periods of time, such as the Indo-European type, as Whorf did speak of it. We need a typology which will let us pick out fundamental features like these. Whorf's ideas, while they have a long way to go, are not hopeless. There is a solid content to them.

7.2.2. Discussion. Smith stated that if the introducer feels Whorf tried to say language influences thought, or the reverse, in the way Indo-European languages stretch out sentences on this kind of frame, he cannot agree. Whorf was trying to say that language is one of the screens through which experience affects the individual and thus affects his perception. He did not say the Indo-European way of structuring language would bring forth science, but only that a different kind of verb system of structuring would have facilitated modern science. Wells replied with three quotations from Whorf's "Linguistics as an Exact Science", regarding Indo-European structure as the antecedent cause of the world view of modern science. Smith commented that Whorf was saying modern science developed in spite of the Indo-European structuring of experience. We are getting out of it as we become aware that much of our science is out of touch with reality, because of the new way language structures experience. We have re-examined many problems in the light of knowledge of language structure. Only recently have we gotten out of the mind-body problem. Wells replied that the commonest criticism of Whorf is that people can't make out what he is claiming; sometimes it is that language is basic to our whole thought, sometimes that it is only one factor. Concerning what would be evidence that science grew up "in spite of" the struc-

ture of Indo-European, this would be if thought developed after the language was criticized. The evidence regarding Occam contradicts this. Physics grew "regardless" of the language. Jakobson also said one must be very cautious with the application of Whorf's principles. Of course, formal categories in language, when obligatory, reinforce elements in every day experience. These things are not at all projected into our logical operations. These categories do not, for example, say certain words must denote act, or action, or goal. Their whole function is the distribution of entities as if denoting one or the other. The same word, "walk", may occur in all three positions. They influence strongly operations where emphasis is on the expression, the language. A beautiful example where a supposed logical category is actually on the level of language is St. Anselm's ontological proof of the existence of God. It is impossible to translate this into Russian, and the argument thus disappears from Russian theology. The influence of language on science has been overstated. One must not forget that from the beginning of Greek philosophy there were many extremely penetrating discussions of the relations between epistemology and language, and a sharp distinction drawn between epistemology on the one hand and the grammatical categories of language on the other in the classical tradition. At the rise of modern science in the thirteenth and fourteenth centuries, there are several thinkers who make a sharp delimitation between epistemological problems and linguistic categories. There is no direct influence of the syntactic structure of Indo-European languages on scientific thought. Many dangerous conclusions have been drawn concerning the supposed absence of the copula in Russian. Actually, the overt form of the verb "to be" has simply been replaced by a zero form. One question which is puzzling comes from comparing Indo-European languages to other languages of the world, and noting that, in the great majority of other languages, the construction of linguistic signs is much more selective than it is in the Indo-European verb. Many features of the verb permit a sharp differentiation between form and content. Is there no relation between this activity and the philosophy of the Indo-European cultures which emphasize form on the one hand and content on the other? Bidney remarked that Whorf, like Cassirer, tended to set up a closed system of language and culture, based on a cultural idealism. If one disagrees with Whorf, one should get rid of the term "meta" in metalinguistics as now used, and make it an ontological category as it originally was. Also, Jakobson has done injustice to the concept of the ontological proof, which has a long history in philosophy. It should be treated on its merits as an argument, even though rejected. Garvin said that language certainly shapes a child's learning to differentiate his experience. As a reflection of cultural conceptions, language is more important on the folk than on the urban, scientific level. In Kutenai, there are changes in which Kutenai syntax is being replaced by English categories among younger speakers. The major culture areas of the world ought to have some linguistic traits in common, more subtle things, such as certain types of syntactic construction, style, and the like. Wells said one can classify the ways speakers of a language use it to structure experience into (1) judgments of relevance, (2) categorization, and (3) judgments of similarity. He proposed this as a classifica-

tion based on Whorf's examples, making them more explicit. Hahn objected that the Indo-European sentence was not necessarily composed of an actor-action category, nor of a subject-predicate, if predicate implied presence of a verb. For example, the copula is not used in Hittite, nor does Latin require the copula on all occasions. Thus, Latin shows traces of something perhaps present very early. One ought not to set up actor-action as the Indo-European structure of the sentence. Sommerfelt added that, even in Europe, the types given by Wells are not always present. Bar-Hillel then spoke of the need to distinguish between language and linguistic description. There are two problems, (1) that the way of thinking of somebody might be influenced by his language, and (2) that his way of thinking might be influenced by the way he has been taught to think of language. Language also may restrict imagination to a certain degree, have a certain negative influence, but one would not expect it to stir the imagination. Further, there are always means for going beyond the restrictions of the language we use, although one language might be less restricting than another. Osgood questioned the notion that language determines preception, since the exact reverse may be true. It can be demonstrated that long before a child is either encoding or decoding linguistically, every bit of its environment is already categorized perceptually. A child could not possibly wait until it had labels for "mother", "hand", etc., before establishing complex significance for them. Thus, in much of basic culture, experience determines the way language categorizes. Garvin added that children tend to generalize much more than adults, and, as they learn to speak, they learn more differentiation, which depends on culture.

8. Results of the Conference

8.1. From the point of view of anthropology.

8.1.1. Introduction (Lévi-Strauss) constitutes Chapter One of this report.

8.2. From the point of view of linguistics.

8.2.1. Introduction (Jakobson) constitutes Chapter Two of this report.

THE FOLLOWING ABBREVIATIONS ARE USED

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| AA | American Anthropologist |
| AAA-M | American Anthropological Association, Memoirs |
| AA _n | American Antiquity |
| AES-M | American Ethnological Society, Monographs |
| AES-P | American Ethnological Society, Publications |
| AJPA | American Journal of Physical Anthropology |
| AMNH-AP | American Museum of Natural History, Anthropological Papers |
| BAE-B | Bureau of American Ethnology, Bulletin |
| BAE-R | Bureau of American Ethnology, Report |
| CDM-M | Canada Department of Mines, Geological Survey, Memoirs |
| CIW-P | Carnegie Institute of Washington, Publications |
| CU | Columbia University, Contributions to Anthropology |
| CUA-AS | Catholic University of America, Anthropological Series |
| FMNH-AS | Field Museum of Natural History, Anthropological Series |
| ICA-P | International Congress of Americanists, Proceedings |
| IHS-PRS | Indiana Historical Society, Prehistory Research Series |
| IJAL | International Journal of American Linguistics |
| IUPAL | Indiana University Publications in Anthropology and Linguistics |
| JAF | Journal of American Folklore |
| JSAP | Journal de la Société des Américanistes de Paris |
| Lg | Language |
| MAIHF-C | Museum of the American Indian, Heye Foundation, Contributions |
| MAIHF-IN | Museum of the American Indian, Heye Foundation, Indian Notes |
| MAIHF-INM | Museum of the American Indian, Heye Foundation, Indian Notes and Monographs |
| MAUM-OC | Museum of Anthropology of the University of Michigan, Occasional Contributions |
| PA | Pennsylvania Archaeologist |
| PHC-P | Pennsylvania Historical Commission, Publications |
| PM | Primitive Man |
| PMAAE-AR | Peabody Museum of American Archaeology and Ethnology, Annual Report |
| PMAAE-M | Peabody Museum of American Archaeology and Ethnology, Memoirs |
| PMAAE-P | Peabody Museum of American Archaeology and Ethnology, Papers |
| PMCM-B | Public Museum of the City of Milwaukee, Bulletin |
| RAI-J | Royal Anthropological Institute of Great Britain and Ireland, Journal |
| SAA-M | Society for American Archaeology, Memoirs |
| SIL | Studies in Linguistics |
| SWJA | Southwestern Journal of Anthropology |
| SWM-P | Southwest Museum, Papers |
| TAPS | Transactions of the American Philosophical Society |
| TCLP | Travaux du Cercle Linguistique de Prague |
| UC-AR | University of California, Anthropological Records |
| UC-PAAE | University of California, Publications in American Archaeology and Ethnology |
| UCPL | University of California Publications in Linguistics |
| UMPL | University of Michigan Publications, Linguistics |
| UNM-B | University of New Mexico, Bulletin, Anthropological Series |
| UPAP | University of Pennsylvania, Anthropological Publications |
| UPMAP | University of Pennsylvania, University Museum, Anthropological Publications |
| UWPA | University of Washington, Publications in Anthropology |
| VFPA | Viking Fund Publications in Anthropology |
| WA | Wisconsin Archaeologist |
| WDWLS | William Dwight Whitney Linguistic Series |
| YAS | Yale Anthropological Studies |
| YUPA | Yale University Publications in Anthropology |

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As Described by Themselves (119 pp., 1949) \$2.00.

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C. F. Voegelin and Thomas A. Sebeok, Results of the Conference of Anthropologists
and Linguists (67 pp. 1953) \$1.00

Orders for Memoirs may be sent to Erminie W. Voegelin, Manager, International Journal of American Linguistics, Indiana University, Bloomington, Indiana.